GEORGIA INSTITUTE OF TECHNOLOGY
OFFICE OF CONTRACT ADMINISTRATION
SPONSORED PROJECT INITIATION

C618

Date: October 26, 1977

Project Title: Intellectual Mobility, Mentorship, and Confluence in Biomedical Problem Domains: The Case of Reverse Transcriptase

Project No: G-43-611

Project Director: Dr. Daryl E. Chubin

Sponsor: National Science Foundation; Washington, D. C. 20550

Agreement Period: From 9/1/77 Until 2/28/79*
*12 months proposed period plus 6 months for flexibility

Type Agreement: Grant No. SOC77-11593

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$36,000 Total

Reports Required: Annual Technical Letters; Summary of Completed Project; Final Technical Report

Sponsor Contact Person (s):

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Defense Priority Rating: none

Assigned to: Department of Social Sciences (School/Laboratory)

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CA-3 (3/76)

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SPONSORED PROJECT TERMINATION

Date: 3/13/79

Project Title: *Intellectual Mobility, Mentorship, and Confluence in Biomedical Problem Domains: The Case of Reverse Transcriptase*

Project No: *G-43-611*

Project Director: *Dr. Daryl E. Chubin*

Sponsor: *National Science Foundation*

Effective Termination Date: *2/28/79 (Grant Expiration)*

Clearance of Accounting Charges: *by 2/28/79*

Grant/Contract Closeout Actions Remaining:

- _ Final Invoice and Closing Documents
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FINAL PROJECT REPORT
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PART I–PROJECT IDENTIFICATION INFORMATION

1. Institution and Address	2. NSF Program	3. NSF Award Number
Georgia Institute of Technology Atlanta, Georgia 30332	Science Policy Research	SOC77-11593
	4. Award Period From 1 Sept 77 To 28 Feb 79	5. Cumulative Award Amount $34,200

6. Project Title

Intellectual Mobility, Mentorship, and Confluence in Biomedical Problem Domains: The Case of Reverse Transcriptase

PART II–SUMMARY OF COMPLETED PROJECT *(FOR PUBLIC USE)*

To examine the interrelation of cognitive and social structures within biomedicine, the intellectual and political events which resulted in the formation of a problem domain were reconstructed. From the literature culminating in the 1970 discovery of the enzyme reverse transcriptase (RT) and its elaboration during the post-1971 National Cancer Act years, key research and administrative personnel located at the National Cancer Institute (NCI) and various academic laboratories in the U.S. were identified and linked through coauthorship and citation networks. Sampling of these networks led to intensive interviews with fifteen scientists, including the former director of NCI and the two Nobel laureate co-discoverers of the enzyme, and to a demographic analysis of 99 RT researchers based on solicited vitae, biographical profiles, and measures of research productivity. These data support the hypotheses that (1) the problem domain was formed by a confluence of ideas carried by scientists trained in an array of biomedical disciplines, (2) mission-oriented research programs such as those funded by the war on cancer inflate the role of large laboratories (as depicted in the literature) which exploit the basic-science discoveries made in smaller labs, (3) biomedical researchers are intellectually mobile, i.e., they tend to specialize in two or more related areas simultaneously or concentrate their publication in one domain for short (2-5 year) durations, and (4) oral history, as reconstructed through interviews, yields far richer and diverse perspectives on the research process than the written history found in public documents, e.g., the technical literature. In sum, the organization, transfer, and transformation of knowledge in biomedicine follow an "inner logic" that seems to resist both policy intervention and conventional social science analysis.

PART III–TECHNICAL INFORMATION *(FOR PROGRAM MANAGEMENT USES)*

1. ITEM *(Check appropriate blocks)*	NONE	ATTACHED	PREVIOUSLY FURNISHED	TO BE FURNISHED SEPARATELY TO PROGRAM	
				Check (✓)	Approx. Date
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b. Publication Citations		x			
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e. Technical Description of Project and Results		x			
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2. Principal Investigator/Project Director Name *(Typed)*	3. Principal Investigator/Project Director Signature	4. Date
Daryl E. Chubin	Daryl E. Chubin	18 Dec 78

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FINAL TECHNICAL REPORT

INTELLECTUAL MOBILITY, MENTORSHIP, AND CONFLUENCE IN BIOMEDICAL PROBLEM DOMAINS: THE CASE OF REVERSE TRANSCRIPTASE

By

Daryl E. Chubin

Prepared for

THE NATIONAL SCIENCE FOUNDATION

GRANT NO. SOC77-11593

November 1978

GEORGIA INSTITUTE OF TECHNOLOGY

DEPARTMENT OF SOCIAL SCIENCES

Atlanta, Georgia 30332

INTELLECTUAL MOBILITY, MENTORSHIP, AND CONFLUENCE IN BIOMEDICAL PROBLEM DOMAINS: THE CASE OF REVERSE TRANSCRIPTASE

Final Technical Report
to the
National Science Foundation
on Grant No.
SOC77-11593

Daryl E. Chubin

Department of Social Sciences
Georgia Institute of Technology
Atlanta, Georgia 30332

November 1978

SUMMARY

To examine the interrelation of cognitive and social structures within biomedicine, the intellectual and political events which resulted in the formation of a problem domain were reconstructed. From the literature culminating in the 1970 discovery of the enzyme reverse transcriptase (RT) and its elaboration during the post-1971 National Cancer Act years, key research and administrative personnel located at the National Cancer Institute (NCI) and various academic laboratories in the U.S. were identified and linked through coauthorship and citation networks. Sampling of these networks led to intensive interviews with fifteen scientists, including the former director of NCI and the two Nobel laureate co-discoverers of the enzyme, and to a demographic analysis of 99 RT researchers based on solicited vitae, biographical profiles, and measures of research productivity. These data support the hypotheses that (1) the problem domain was formed by a confluence of ideas carried by scientists trained in an array of biomedical disciplines, (2) mission-oriented research programs such as those funded by the war on cancer inflate the role of large laboratories which exploit the basic-science discoveries made in smaller labs, (3) biomedical researchers are intellectually mobile, i.e., they tend to specialize in two or more related areas simultaneously or concentrate their publication in one domain for short (2-5 year) durations, and (4) oral history, as reconstructed through interviews, yields far richer and diverse perspectives on the research process than the written history found in public documents, e.g., the technical literature. In all, the organization, transfer, and transformation of knowledge in biomedicine follow an "inner logic" that seems to resist both policy intervention and conventional social science analysis.

PREFACE AND ACKNOWLEDGMENTS

The cooperation and assistance of several people have enhanced this project substantively and operationally, but moreover, made it a most rewarding experience for me. My principal consultant, Dr. Kenneth Studer, was invaluable in the interviewing phase, while Ms. Iris Mitchell has been remarkable in transcribing 25 hours of tape-recorded interviews into more than 600 double-spaced pages of text. The material comprising this report was conscientiously typed by Ms. Pamela Stembridge and Mrs. Gwen Nance. Never failing to provide sound advice and administrative information was Mr. James Camp. Document retrieval, xeroxing, data coding, and computer programming were ably performed by two Georgia Tech Industrial Management students, Mr. Stanley Muse, and later, Mr. Steven Shelley. The encouragement of local colleagues is always an asset; hence I am indebted to Mr. Jon Johnston and to Drs. Patrick Kelly, Morris Mitzner, Alan Porter, Frederick Rossini, and Jay Weinstein. Other confidants whose kind words have helped sustain me this past year were Dr. Ian Mitroff of the University of Pittsburgh, and Dr. Stephen Turner of the University of South Florida. My wife Vicki, as usual, has been a loving sounding board. Finally, my thanks go to a patient and dedicated cadre of scientists who indulged this social scientist by interrupting their busy schedules, and then took an active interest in candidly sharing their insights so that I might realize my research goals. The commitment of each to recapture in 90 conversational minutes the history they shaped made for exciting and informative encounters that no student of science should forgo. Thank you Mr. Louis Carrese and Drs. Stuart Aaronson, John Bader, David Baltimore, Joseph Beard, Gary Gerard, Raymond Gilden, Maurice Green, Robert Huebner, Frank Rauscher, Jeffrey Schlom, Sol Spiegelman, Howard Temin, George Todaro, and Sue Yang.

Two publications have already resulted from this project, both coauthored by Dr. Studer and myself:

"The Politics of Cancer." Theory and Society 6 (July 1978): 55-74;

"Knowledge and Structures of Scientific Growth: Measurement of a Cancer Problem Domain." Scientometrics 1 (January 1979): 171-193.

A book manuscript, tentatively entitled Viruses and Cancer: A Case Study of Growth and Specialization in Biomedical Research, is nearing completion. A few publishers have expressed interest in reviewing it in its entirety. At least one other paper, probably utilizing excerpts from the interviews, will be submitted for publication within the next quarter year.

Atlanta
November 1978

Daryl E. Chubin

INTRODUCTION

This is the final technical report on Grant No. SOC77-11593 awarded by the Science Policy Research Program in the Social Science Division of NSF. The grant provided one year of support, beginning in September 1977, although two years were requested in my proposal submitted by the University of Pennsylvania in January 1977, and resubmitted by the Georgia Institute of Technology in June 1977.

With this grant I (a) collected biographic and bibliographic data through on-site interviews, a mail solicitation of curricula vitae, and document retrieval, as well as (b) analyzed and consolidated these data with information compiled in earlier phases of the study (conducted under the sponsorship of an NIH contract when I was a Research Associate in Cornell University's Program in Social Analyses of Science Systems). My activities and findings derived from the NSF phase of support are detailed in the body of this report. A note on what was not accomplished in this one-year project, however, is appropriate here.

Despite "mentorship" in the project title, this aspect of the study, conceptualized genealogically in the proposal, had to be sacrificed. There simply was no time to link mentors and students within and beyond the reverse transcriptase domain in the systematic fashion I had envisioned. Some evidence of the linkage between lab directors and postdocs was unearthed and is duly reported below (see "Demography"). But the time constraint precluded the extensive interviewing of more geographically dispersed scientists to discuss the mentor-student relation and to mine interviewees' local documentary sources for formal clues. Thus this study, and the wealth of information I suspect it would yield, remains undone.

That the interviewing was so successful, but restricted to key researchers and administrators, represents a regrettably unfulfilled aspect of the project as proposed. Securing the hypothesized range of opinion sought from scientists

"weakly tied" to the domain, i.e., structurally peripheral or only transiently linked to its major coauthorship or citation networks (see "The Interviews"), was not feasible in the approved time frame and budget. Consequently, no travel occurred west of the Mississippi River, specifically, to several west coast centers and researchers, e.g., University of California Medical School in San Francisco, and the Bishop-Varmus team. Of course, the same lament could apply to our exclusive U.S. focus; foreign travel to centers in the U.K., Israel, and West Germany was impossible. Though this domestic focus was justified, the potential of sampling perspectives from the international cancer community nonetheless had to be deferred. What makes this lost or "deferred" opportunity so personally frustrating is that a project that is international in scope could provide additional evidence of confluences in biomedical domains.

Indeed, the notion of intellectual problems cross-cutting disciplines and even ongoing research programs with a mission or targeted orientation has been supported. But empirically, the mechanisms of convergence or confluence lack specificity, even though "intellectual mobility," migration between problem domains, or inter-disciplinarity appears to be theoretically viable. It may be applicable to the physical sciences as well, as recent studies (Edge and Mulkay, 1976; Friedkin, 1978) attest. Much more and varied data are needed to move from metaphor to model, as it were, for as one referee of the project proposal observed, "Confluence of rivers is straightforward; confluence of [intellectual] influence is problematic at best."

If such criticism is taken to mind, then it joins other problems identified for further study which antedated and have outlived this project, or have been generated by it. Of the former variety the problem of defining, identifying, and/or circumscribing specialties, clusters, networks, or domains for study persists. Some degree of arbitrariness is inevitable (Woolgar, 1976a), but the lack of

comparability in what we call and how we measure our units of analysis confounds the task significantly. Because conceptual (im)precision underlies operationalization and measurement procedures, the prescription of Blume (1977) and Spiegel-Rösing (1977) for theoretical rapprochement and multiple methodological approaches must be heeded. If we are to bridge various science studies, the tradeoffs in utilizing concepts, measures, and data sources that are near and dear, i.e., familiar and manipulable, must be explicitly addressed.

Related to this problem is the one of generalizability. How far can we reasonably generalise the findings of a case study? What is "reasonable," especially in the present case, when the study is at a sub-specialty or problem domain level? My reply is that these may be the wrong questions to ask, particularly with biomedicine as the referent "cognitive region" (Böhme, 1975; also see Report of the President's Biomedical Research Panel, 1976). For reverse transcriptase research (i.e., a domain) may be representative of cancer virology or cell transformation (specialties), unrepresentative of virology (a discipline) and cancer research in general (a supra-discipline or broad multidisciplinary field), but typical of how the war on cancer affected research and collaboration within the laboratory (i.e., local teams) or within the government sector ("in-house" at NCI). In short, the comparability of, as well as the basis for generalizing from, biomedical case studies reside in the rationality or "inner logic" of this cognitive region of science.

Consider this further: in all its singularity, a problem domain reflects the intellectual, organizational, and political properties of the contexts in which it is embedded. True, the reflections may be distorted, but this is why triangulated measurement is indispensable in science studies. Indeed, in micro-level case studies the need for triangulation may seem imperative. I would argue, however, that macro-level analyses, too, require such "validation." For example,

Narin's (Narin et al., 1976) bibliometric typology of research activity purports to distinguish "basic" from "clinical" publication as well as the flows between such categories--its utilization in other discipline- and problem-bound literatures. But the typology seems to assume uniform (unchanging over time) content of journals, and utilization is measured by aggregate citation counts. Thus this macro perspective on journal relationships within the biomedical literature is built upon a defensible but assailable "influence map" methodology. Though providing diagnostics for policy purposes and heuristics for research, this methodology is insufficient for explaining such things as the formation of biomedical domains, citation patterns within RT, and other relationships at the non-institutional level.

The point is that generalizing the findings from a macro methodology to the micro level is simply not warranted. This is why the problem of scope must be resolved by the rapprochement of multiple and complementary approaches to biomedical science. Yet in so doing, the nature of the research task is transformed. Indeed, the empirical "discrepancies" yielded by various approaches to the same region of science shift the task from one of supplying answers to that of reporting the range of perspectives different methodologies tend to evoke. So perceived, both the analyst and the policy-maker must weigh and extract meaning from assorted evidence rather than legitimate and interpret as definitive the findings derived from any one approach. The task is thus conceived as one of approximating and negotiating reality (see Lemaine et al., 1976; Edge, 1977).

When the interdependence of intellectual and social realities is examined in the context of application, e.g., disease prevention and control or the discovery of causal agents, the policy or "external" social dimension looms large (see Williams et al., 1976). And so it was in the reverse transcriptase domain; a study of growth of a research community was transformed into a broader inquiry of funding priorities and mechanisms as they impinge on the organization, conduct, and

politicization of research (see "The Politics of Cancer").

Suffice it to say that the ensuing knowledge claims are only partially represented in and by the technical literature of a domain; this public record is a social form that is rhetorical and self-serving (Gilbert, 1977; Knorr, 1977). Oral history, as created in interviews, is no less rationalizing, vulnerable to poor memory and recollections that juxtapose or blur detail (though I am far more sanguine about this than Woolgar, 1976b). The key to reconciling these accounts is not to discard or dismiss any of them as myopic or utterly lacking in objectivity. Each lends a perspective on scientific reality that describes products, structures, and the "state" of the reality, e.g., science indicators (see Elkana et al., 1977), or the processes whereby knowledge claims are produced and negotiated. It is noteworthy that the traditional policy emphasis has been on products; perhaps the micro analyses found in the burgeoning "specialty" literature will enhance our understanding both of how science gets done and what its payoffs are.

In the present case, interviewing of protagonist-informants rendered insights into process--recapturing the meanings they attributed then and now as the state of knowledge and their respective research programs advanced--that were otherwise inaccessible. It was precisely my belief in the "force of knowledge," its incomplete if not misleading measurement, that led me into conversation with participant reverse transcriptase researchers; away from those conversations I came with new grounds for interpreting research processes in a domain of Big Biology.

The newest of these "new grounds" are presented in the three chapters that constitute this final report. These chapters, however, were selected from a longer manuscript (still in preparation) on the four-year study of Viruses and Cancer (a Table of Contents follows). The first chapter included here outlines the interview strategy, the second demonstrates the use of the interview data in an expanded version of "The Politics of Cancer" article published

in July 1978, and the third features a demographic analysis synthesizing the career data collected this past year to augment the intellectual history of the RT domain. References to chapters not contained in this report occasionally appear. Early revisions of these chapters have circulated as working and presented papers since 1975. But revised versions of these other chapters will be available as part of the book manuscript by the end of this calendar year. Thereafter I will be glad to furnish copies upon request.

VIRUSES AND CANCER: A CASE STUDY OF GROWTH

AND SPECIALIZATION IN BIOMEDICAL RESEARCH

Table of Contents

THE INTERVIEWS

THE METHODOLOGY OF STRUCTURAL INTERVIEWING

Interviewing is anything but a novel data-collection tool in sociology. The selection procedure which precedes such data collection, however, not only targets subjects for interview, but does so for various reasons and according to any of a multitude of designs (see Hyman, 1975). Sampling of networks is quite unlike conventional designs which seek to optimize representativeness, and therefore generalizability, through a random selection procedure.[1] Our reasons for interviewing were neither generalizability (in the statistical sense of making inferences about a population) nor representativeness (in the sense of covering a known distribution of opinion or knowledge). Rather, our concern was to identify and then interview individuals who occupied particular positions in different networks. Herein lies the novelty of our data collection effort.

Because any social network is a structural representation of relationships generated by particular behavior, e.g., authorship, one can derive any number of networks generated by different behaviors which relate the same set of individuals in numerous ways. To compound the sampling problem, any behavior and relation formed by it can be disaggregated over time. Such disaggregation lends a dynamic dimension to the static structural representation. Disaggregation also multiplies the number of network structures, each of which can be thought of as a sampling frame. The choice then becomes whether to sample each frame, say, by year and generating relation, or somehow to juxtapose the frames to reveal patterns of variance and invariance in positions across networks. Structurally, the individual whose

position in a network changes by year and relation may be as "interesting" or valuable for interviewing purposes (if not more so) as one who is "strongly tied" in every network. This is the crux of Granovetter's (1973) perceptive argument that sociometrically there is strength in "weak ties."

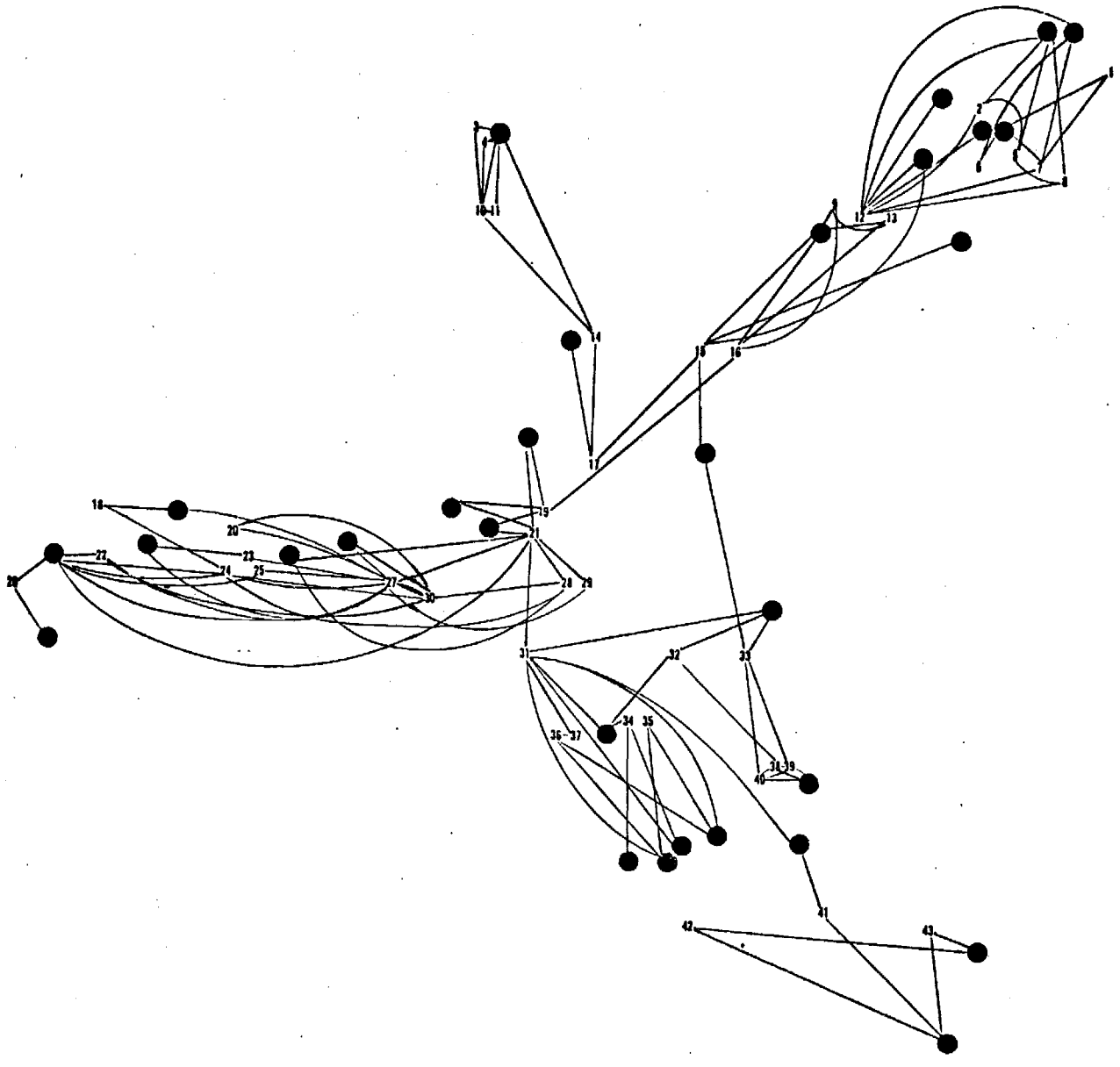
Another consideration is that the tempo by which structural relations change, i.e., evolve over time, may not be accurately captured by a network representation that arbitrarily slices relations into calendar year units. In other words, for the sake of analytical convenience, we may be sacrificing precision in the measurement of structural relations. How, then, does one "preserve" the social process underlying the structural representation by sampling (i.e., "operationalizing") the network?

For sure, one must sample purposively, not randomly, and in the absence of sociometric data, rely on visual inspection of the structures themselves: What are the configurations? Which dyads and triads appear repeatedly? Is their position (or physical location) on the network "map" relatively stable. Do they disappear in some years and on some relations and then reappear? These questions disclose the nature of the task. After studying the pictures[2] of structured relations (e.g., see Figure A1) and noting some of the patterns implied in the above queries, we began to perceive certain individuals and laboratories as important targets for further examination. Combined with our knowledge of intellectual events and principal characters in the narrative history of RT, the structural data offered another perspective (or set of perspectives) on visible researchers and organizational sectors, namely, those emerging from various bibliographic generators of networks, e.g., co-authorship, co-citation, and co-acknowledgment.

Structural sampling must utilize this abundance of information: It must draw on the array of available empirical information to determine

FIGURE A1

R.T. CO~AUTHORSHIP NETWORK, 1973



1	Ting, R.C.Y.
2	Bhattacharyya, J.
3	Swan, D.
4	Packman, S.
5	Sarin, P.S.
6	Paran, M.
7	Wu, A.M.
8	Reitz, M.S.
9	Livingston, D.M.
10	Aviv, M.
11	Leder, P.
• 12	Gallo, R.C.
13	Lieber, M.M.
14	Rosa, J.
15	Todaro, G.
16	Benveniste, R.E.
• 17	Aaronson, S.A.
18	Filatov, F.P.
• 19	Scolnick, E.M.
20	Tershov, F.I.
• 21	Parks, W.P.
22	Ilyin, K.V.
23	Mazurenko, N.P.
24	Bektemirov, T.A.
25	Irlin, I.S.
26	Uryvaev, L.V.
• 27	Zhdanov, V.M.
28	Miller, G.G.
29	Solov'ev, V.D.
• 30	Bykovskii, A.F.
• 31	Gilden, R.V.
• 32	Vernon, M.L.
• 33	Huebner, R.J.
34	Sarma, P.S.
35	Long, C.W.
36	Hatanaka, M.
37	Okabe, H.
38	Duh, F.G.
39	Cho, H.Y.
• 40	Rhim, J.S.
41	Rongey, R.W.
• 42	Gardner, M.B.
43	Roy-Burman, P.

who should be selected for interviewing.[3] Targets may be selected, therefore for their consistency of position in different networks, their centrality in some, their peripherality in others, their organizational setting, and their links to others in the relevant research populations. The rationale for sampling according to multiple criteria is that structural positions and relations are assumed to condition one's perceptions of the phenomena in question. What structural sampling seeks to tap is the divergence of perspective that participation in a social system, e.g., a formal communication network, entails. Interviewing the designated targets of a structural sample will presumably develop insights about not only one's participation in the network, but also one's perspective on whether the network really exists, how it has evolved, and why.

Structural interviewing operationalizes the notion of "triangulation" (Webb et al., 1966). The ability to approach one's subject in a myriad of ways should indeed seem to be the key to faithful social reconstruction of reality--in this case a problem domain or research area within biomedicine That social scientists would rather invest in single indicator approaches than sort and merge multiple indicators and perspectives prompted our recourse to an interview strategy derived from structural data. Intrinsic to any kind of social data, structural or otherwise, is sufficient measurement error to render findings and interpretations based upon them dubious at best. Rather than belabor or bemoan this point, it would seem far preferable for social scientists to assume its truth and adopt a skeptical, but constructive, measurement posture: Treat data as a tentative baseline, as an approximation to refine and on which to build with the collection and analysis of other data.

Structural interviewing does this in two ways: It builds on structure

(i.e., generated networks), and it recognizes the credibility of one's perspective on the structure, his/her role in creating it, sustaining it, etc., without attributing credence to views based on structural position. Credence stems from the collection of views, the assimilation of perspectives from different vantage points in the network(s). In short, structural interviewing is a "convergence technique" (Carrese and Baker, 1967). It accords primacy to no datum, while it respects all. It does subordinate social measurement to the chronology of historical fact, but it neither disqualifies nor disarms the social scientist from fashioning explanations and interpretations. Indeed, structural interviewing allows the "outsider" to glimpse through "insider's" eyes without becoming either blinded by insider's vision, or resolutely myopic as outsiders (particularly social scientists studying natural scientists) tend to be.

As defensible as the rationale for structural interviewing might be, the methodology itself still lacks the elegance to be called, and readily applied as, an algorithm. Perhaps a comparison with an imperfect, yet tractable, methodology-cum-algorithm in science studies, analysis of co-citation clusters, will illustrate the promise of structural interviewing.

Co-citation analysis establishes linkages between pairs of documents listed in the bibliographies of articles in some citing literature. The resulting set of cited documents represents a "map of science" never before visible in the research literature (see Small, 1973; Small and Griffith, 1974 ; Griffith et al., 1974). These retrieved documents cluster in various subject areas of science, and thus reveal the structure which connects subjects as well as those documents which dominate in a certain area.

Elsewhere (Chubin, 1976: 451f), I have questioned the assumptions of co-citation analysis. Germane to the present discussion, however, is

recognition that the co-citation cluster is (1) a purposive sample of literature which (2) lacks an inherent decision rule for determining significantly visible documents. That is, "visibility" is an arbitrary threshold of citations relative to the total number any pair of documents might garner during some designated period. As a diagnostic tool, co-citation analysis, according to its proponents, can identify "hot" areas of research. The sole attempt to confirm this hypothesis (Small, 1977) has been encouraging. Nevertheless, if co-citation analysis is used to guide selection of potentially important areas for research, citation and the "implicit theory" (Mulkay, 1974) that underlies it become decisive; subject matter becomes secondary. Intellectual issues and organizational settings are treated as relatively invariant in time and manifestation despite citation "eccentricities" to the contrary* (Price, 1970; Meadows and O'Connor, 1971; Sullivan et al., 1977).

Co-citation analysis, to me, anyway, seems fraught with unknowns; its assumptions strike me as plausible but unsound. Indeed, as a diagnostic it inverts the order of analysis by methodologizing the content of science, shackling its intellectual manifestations to a single indicator--the citation or bibliographic reference. And as a higher-order datum, the co-citation--a purposeful listing of documents in combination--is a proxy for communication, information, allegiance, deference, etc. But why begin with a proxy when its antecedents are accessible and knowable?

Structural interviewing reorders the approach to scientific "objects." It begins with a known subject matter and is a proxy for nothing except the clearly unfeasible alternative of interviewing a population. It derives from multiple networks or clusters of co-relations. And, above all, it is a methodology blinded a priori neither to intellectual idiosyncrasy nor by

*As an eleventh-hour addendum, I can report further experimentation with co-citation thresholds (in the area of "opiate receptors") by Small and Griffith at the Third Annual Meeting of the Society for Social Studies of Science at Indiana University, Bloomington, Indiana (3-5 November 1978).

convenience. Structural interviewing is a culmination of other component structural analyses. As a small sample technique, it requires the findings of such analyses, e.g., of co-citations, to inform both the actual sampling, and the interviewing itself.

Our sampled RT targets and their respective organizational sites are summarized in Table A1. Again, the interviews were intended to elicit recollections and subjective perceptions to compare with other observations--not only those of other interviewees, but with bibliographic network data and historical documents as well.

Thus, we claim that as a retrospective technique, structural interviewing endows the process and product of reconstructing a research area with evolutionary and integrating perspectives. We also concede that perspectives do not an algorithm make; they surely do make for a potent tool in scientific specialty studies, however. What follows is a test of our claim: a description of how the (1) targets were contacted, (2) the interview sessions were conducted, and (3) the methodology of structural interviewing can be used to reconstruct through a composite of protagonists' accounts an oral history of a problem domain.

INITIAL CONTACT

Each interview target (or "subject") was sent a personalized letter in January or February of 1978. This letter described the purpose of the research, its support by an NSF grant, and carried an admission that, after collecting and analyzing substantial bibliographic and biographic data, we as "outsiders" lack the insights that only researchers working in a problem area can provide. Two or three sentences describing our perception of the subject's contribution to RT research followed plus an invitation to share his/her perspective with us in a 1-2 hour "conversation" ("interview" being)

Table A1

Initial Structural Sample of Local Organizational Sites
for Targeted Interviewing of RT Researchers

Local Organizational Site	Interview Targets	
	Primary	Secondary
Government:		
Laboratory of Tumor Cell Biology, NCI	Gallo, Robert C. Smith, R. Graham	-
Viral Carcinogenesis Branch, NCI	Parks, Wade P.	Huebner, Robert J.
Viral Leukemia and Lymphoma Branch, NCI	Aaronson, Stuart A. Scolnick, Edward M. Todaro, George J.	Ross, Jeffrey
Primate/Quasi-goverment:		
Bionetics Research Lab (Litton)	Reitz, Marvin S.	Ting, Robert C.Y. Wu, Alan M. Yang, Stringner S.
Flow Lab	Gilden, Raymond V.	-
Medical School:		
Institute of Cancer Research, College of Physicians and Surgeons, Columbia University	Spiegelman, Sol Schlom, Jeffrey	Axel, Richard Baxt, W. Gulati, Subhash C. Hehlmann, R.
Institute for Molecular Virology, St. Louis University School of Medicine	Green, Maurice	Gerard, Gary F. Grandgenett, Duane P
Academic Departments:		
Department of Biology, Massachusetts Institute of Technology	Baltimore, David Verma, Inder M.	Huang, Alice S. Temple, Gary F.
McArdle Lab, University of Wisconsin	Temin, Howard M.	-

a dreadfully formal term). A stern paragraph urging cooperation with our efforts, as well as our flexibility in accommodating to the subject's busy schedule, concluded the letter. A self-addressed stamped postcard accompanied the letter asking the subject to indicate specific dates within the month (the bulk of the interviews occurred in February and March) which were most convenient. The subject was instructed that soon after receiving this card we would be in contact by phone to confirm a date and time for our meeting.[4]

Table A2 contains the names of the interview targets with whom we spoke and their institutional/organizational sector which helped to frame their selection. A comparison of Tables 1 and 2 reveals that our targets, regardless of sector, were most receptive to our invitation to talk. This receptivity, as the excerpts from the interviews attest, was reflected in their candor, sincerity, and genuine interest in our work. We discuss this happy circumstance below as a prescription for future studies. For now, suffice it to indicate that subject to travel budget and time constraints, we were able to speak with most everyone we had hoped--though, in retrospect, had no grounds for that hope--to see. Indeed, the responsiveness of all, especially NCI personnel, far exceeded our expectations.

Merely from reading Science reports and hearing colleagues express pessimism about the willingness of NCI researchers to discuss the cancer war, we approached this sector with concern, if not trepidation. For this reason, we decided to contact NCI people before we ventured into academic labs, particularly the ones where the discovery of the enzyme had been made. Fortunately, our concern was unfounded. So long as we were "neither from the Washington Post nor named Dan Greenberg," as one of our subjects put it, the meeting was a welcome opportunity; for some it was a veritable catharsis, an unburdening of many of the pressures which highly visible, and therefore,

Table A2

RT Interview Subjects, by Organizational Sites

Sites	Interview Subjects
Government	Aaronson, Stuart Bader, John [a] Carrese, Louis[b] Huebner, Robert Rauscher, Frank[c] Todaro, George
Private	Beard, Joseph Gilden, Raymond[e] Yang, Stringner[f]
Medical School	Gerard, Gary Green, Maurice Schlom, Jeffrey[g] Spiegelman, Sol
Academic Department	Baltimore, David Temin, Howard

[a]Head, Cell Growth Regulation Section, Chemistry Branch, NCI; interviewed on recommendation of early subjects.

[b]Associate Director for Program Planning and Analysis, Office of the Director, NCI, interviewed on recommendation.

[c]Former director, NCI; currently, Executive Vice-President, American Cancer Society; interviewed on recommendation.

[d]Sole supplier (on NCI contract) of Avian Myeloblastosis Virus and purified reverse transcriptase; interviewed as most acknowledged in RT literature; Emeritus Professor of Surgery at Duke University; currently President, Life Sciences Research Labs, St. Petersburg, FL.

[e]Currently at Frederick Cancer Center, Frederick, MD (managed by Litton-Bionetics).

[f]Currently at Laboratory of Cell Biology, NCI.

[g]Currently Head, Breast Cancer Section, Laboratory of Viral Oncogenesis, NCI.

controversial, in-house projects represent (a theme elaborated below).

Two NCI luminaries whom we were unable to see were Robert Gallo and John Maloney. The latter was contacted less than two months after he had been rather unceremoniously removed as head of the Special Virus Cancer Program, the NCI program which, upon review in 1973 by the Zinder Committee, came to be perceived as embodying all the vices of big contract research and few of the virtues. The meaning of Maloney's "promotion" to Assistant Director of the Institute at the request of Arthur Upton was clear to our subjects. They advised we "leave Maloney alone," while Maloney's secretary suggested we talk to his "acting" successor, John Sibal. For various reasons, we declined this option.

The failure to see Gallo was of a different sort. His travel schedule simply did not permit our meeting. A chance to interview a long-time associate of his was discouraged by one or two of our NCI subjects who said our persistence to see Gallo would be rewarded ("Gallo is a dynamic guy"; he is also a staunch advocate of contract research and channeling "unlimited" sums to cancer research).

Partly due to our disappointment over missing these targets (though only Gallo was originally targeted), and partly based on our realization that NCI program heads and lab researchers regard NCI administrators, planners, and policy-implementers with a mixture of perplexity and suspicion, we decided that the subjects of such ambivalence deserve close attention. Our choices, though not necessarily representative, were outstanding: Frank Rauscher, former NCI Director, and Louis Carrese, Associate Director for Program Planning and Analysis. Carrese was particularly receptive to our questions and spoke in animated fashion for 2½ hours about the incredulity harbored by NCI researchers about his office. There was a vindicating, yet compassionate, tone to this

interview. In fact, Carrese said that he would encourage an assessment of the planning and policy which has emanated from his office for the last 15 years; it would reduce much misunderstanding about his "systems" approach as well as his mediating role vis-à-vis Congress and NCI researchers.

Rauscher, now an Executive Vice President of the American Cancer Society, and unanimously respected by the NCI researchers we interviewed, is the epitome of public relations. However, he was anything but evasive; he is circumspect and demanded that we discuss the ultimate purposes of our study before he allowed us to switch on the cassette recorder. Once satisfied, he produced a fluid 90 minutes of information and pledged to assist us in securing some important documents which had eluded us for months. Like many of our subjects, he volunteered to read select portions of the manuscript and asked to be kept informed of the study's progress.

Without exception, in our in-person contacts we sustained the good impression which our letter of inquiry about the interview had apparently established. Within three weeks of each interview, we sent a note of thanks to each subject for his/her cooperation and of assurance that we would keep them apprised of, and indeed enlist their skills to evaluate, our findings. Like good anthropologists, we returned "from the bush" with the good will of our tribe of subjects intact, a situation we strived to guarantee by our behavior prior to, following, and during the interviews. How this was done in the "during" protion of the interviews is the next topic for review.

THE INTERVIEW SESSIONS

The interviews took place in the office or lab of the subject. With one exception, the subject was engaged in a three-way conversation with both the PI and his principal consultant. Each session was audio-taped with the permission of the

subject who was informed that he or she would be sent for review a copy of material containing quoted excerpts from the interview. In this way, both the accuracy and the context of quoted statements would be preserved.

None of the subjects balked at our recording procedure, though several used the apparatus to differentiate "on the record" from "off the record" commentary. On occasion, we were asked to "switch off" in preparation for an especially candid, and sometimes ugly, remark. Naturally, we complied with such requests. Sometimes we were surprised when sensitive remarks warranted such requests, but no request was made.

In all cases, one of us (D.E.C.) spent 5-7 minutes recounting the origin of the study, its guiding questions, and progress to date. This initial explanation, of course, was less a communication of information than an ice-breaking, tone-setting device. As we became more adept at tailoring our introductory remarks to the role of the subject's work in the RT saga, we succeeded in establishing rapport earlier in the conversation. The one element contributing to the rapport, however, was intellectual--our conversance with the science of the subject's research. Our ability to discuss experimental and theoretical issues, e.g., the provirus hypothesis and the unreliability of inhibitor data, made us credible, curious social scientists who had taken our subject matter and its creators very seriously. This ability to enter the biologist's world of discourse requires ample preparation--essentially an understanding of primary source materials, and awareness of historically significant events (not just discoveries, but conferences and less official public exchanges), and a knowledge of researchers' whereabouts, collaborators, and programs at various times. Without this reservoir of pertinent facts, one simply cannot ask pertinent questions.

This is one critical difference between social networks and structural

interviewing. Networks can help identify interview targets; they are of little help in determining what those targeted people should be asked. Structural interviewing results from a cumulation of information about targets and those topics they are singularly equipped to comment upon. The opportunity to interview should not be squandered on sociometric or structural questions (which in the abstract are not of gripping concern to the subject or his science). Such an approach nestles the interviewer comfortably in his own world of sociological discourse and obviates the need to probe for intellectual bases of the behavior represented in a social network.

Structural interviewing surmounts, or at least erodes, the intellectual barrier between the natural scientist and his social scientist-interrogator. Our subjects sensed our effort to surmount this barrier. In response, many became an "open book"; indeed, some seemed grateful for the questions of outsiders who could understand enough, but not so much as to be a threat. In short, we were taken into the confidence of almost all of those with whom we spoke.

After opening with queries about whether the subject was surprised by the discovery of RT, what his or her research focus was at the time, and whether this focus changed soon after the discovery, we eased into discussion of the subject's organizational setting (e.g., the structure of his lab, division of labor within it). The conversation then gravitated to the question of antagonism between NCI and academic labs. This was our typical point of departure into the policy area (e.g., effects of the war on cancer on one's research and lab structure, and reaction to the Zinder Committee report criticizing in general the contract mechanism in NCI, and particularly the Special Virus Cancer Program). This is not to say that the interviews were so structured that topics were not addressed in other sequences. They were.

One of us would have a one-page schedule of topics to be covered, and in no case was a topic untouched for lack of time. Questions raised by observations made by the subject were followed up either as they occurred or after the schedule was exhausted.

As anticipated, we found the interviews to be quite intense. The advantage of our both being present at the interview was clear inasmuch as one of us would pursue a line of questioning, allowing the other to formulate and phrase questions privately; the other would then enter the dialogue anew. The mental concentration required to process answers and prepare related questions was exhausting indeed. In most cases, both we and the subject were drained after 90 minutes.[5]

Near the end of the interviews, the subjects were asked to suggest the names of colleagues whose insights they valued and who, they thought, would be willing to speak to us. It was overlap in these "snowballed" names that led us to NCI administrators, but confirmed that our selection of literature-based interview targets was well-founded.[6]

A final note: There is a "learning effect" which takes place when successive interviews are conducted during a period of only a few weeks. This effect surely polishes the syntax, phrasing, and delivery of questions, but it also heightens the interviewers' anticipation of replies to those questions. When one gains facility with one's research instrument, there is a need to compensate for inflections and facial expressions which may inadvertently cue the subject that "I'm curious as hell about your response, but I think I know what you're going to say," or "I've asked this delicate question of all the previous subjects; now how forthright are you going to be?". The way we compensated for such stirrings was to preface the question with a naive remark such as, "Because we are not formally trained as biologists . . ," or

"We realize now that the Cancer Institute is so vulnerable; it is constantly in the public eye, yet it seems to have unduly attracted much bad press . . ," or, stronger yet, "We were frankly critical of the NCI and hence deliberately chose to interview NCI personnel before visiting the academic researchers. . . ." Whether our "staged naivete" was convincing is unknown, but we suspect that the subject, upon hearing such an admission, was more inclined to share a confidence that he ordinarily would not. This is pivotal in the interviewing of scientists: You must give a little to get a little. You must strike a balance between credible and informed interviewer on the one hand, and confidant and respectful seeker of valuable new information, on the other. Above all, you must doggedly pursue the subject who, in the same interview, can adopt the posture of public relations man, intellectual, healer, bureaucrat, visionary, friend, and adversary. As interviewers, we became sensitive to such changes in character; we tried to adapt accordingly. Tellingly, refusal to answer a question was rare; smoke-screen was more frequent, but with two interviewers and 90 minutes or more of conversation, questions can be reworded, digressions can be stymied, rapport can build. In very few instances did we feel our control over the situation slipping. Even those few subjects who sought to set the boundaries for the discussion at the outset of the interview became less defensive, their answers more fluent. They carried the discussion; we merely clued them into the problems and issues we wished to discuss.

The implications of these interviews for future studies appear to us to be far-reaching. In the following section, we compare our experiences with recent and prominent attempts to use scientists as a primary data source in the sociology of science.

PROTAGONISTS' RETROSPECTIVE ACCOUNTS AS A DATA SOURCE IN THE SOCIOLOGY OF SCIENCE

Although the interview--focused, open-ended, or whatever--is a standard sociological mode of data collection, there are few precedents in the sociology of science literature for interviewing scientists as we did. Foremost among the precedents are Edge and Mulkay's (1976) analysis of the development of radio astronomy in Britain, and Zuckerman's (1977) study of American Nobel laureates. Each of these researchers has reflected on their methodology, particularly on the interaction between interviewer and scientist-interviewee (Mulkay, 1974; Zuckerman, 1972; 1977: Appendix A). Because our interview approach is intermediary between these efforts--combining the best of both while resembling Olby's (1974) impressive synthesis of interview material and other informal personal communications with historical documentation--we deem it instructive for future studies to add our experience to this corpus and reflect on interview methodologies.

Underlying our concern for reflection is not merely the reactivity of the interview as a data source. Of course, the interview is a social act and sociologists are wont to make it a self-fulfilling prophecy. But interviewing scientists, whether or not they qualify as "ultra elites" (to use Zuckerman's designation of the laureates), presupposes a theory of data. Our theory coincides putatively with that articulated by Mulkay (1974:110):

> Firstly, if the sociological study of science involves a close examination of its technical culture, the active cooperation of technically competent participants must be gained in one way or another. Secondly, on many issues of sociological interest, members of a given research community are likely to have firm and agreed definitions of reality which are linked to their technical and scientific assumptions. It is often possible to regard such issues as problematic only if the investigator has enough technical knowledge to challenge these firm definitions.

It is at this point that our approach departs from that of Edge and Mulkay.

For they advocate mounting the "challenge" by combining and counterbalancing the perspectives of an insider (in their case, David Edge, trained as a radio astronomer, and, therefore, an ex-participant scientist), and an outsider (Mulkay the sociologist). "Perhaps the best arrangement is to have the scientists interviewed by both a sociologist and a participant/ex-participant" (Mulkay, 1974:114).[7]

But perhaps what is "best" in this domain of science would not be so in another. We found that two outsiders, as indicated earlier, <u>can</u> learn the technical culture, not only to their own satisfaction, but to the subjects' satisfaction. This is confirmed by Zuckerman (1977) who, with embarrassed immodesty, reports some of her subjects' appraisals of her preparation for the interviews:

> You've done your homework, haven't you? (p. 262)
>
> You've read the history pretty thoroughly, haven't you? (p. 263)

Such rhetorical affirmations belie the resistance which social scientists often encounter when they interview scientists. Consider this exchange between Zuckerman and a biochemist:

> L[aureate]: With the arrogance of the scientist, I should say that I don't think it is possible to make a good study of collaboration among scientists . . . unless one has had some work in natural science.
>
> Int[viewer]: Well, as you know, we all labor under certain disadvantages. My own sense is that you have to familiarize yourself as much as possible with the kind of work you're studying.
>
> L: You can't do that very well at a distance. . . . (Zuckerman, 1977:266).

What is so telling about this exchange is that most <u>sociologists</u> of science subscribe to the <u>biochemist's</u> view. Indeed, the eight chapters which precede Zuckerman's "Interviewing" appendix speak louder than her words cited above. The science which led to the Prize or the occupation of the "forty-first chair,"

though competently described, is clearly a secondary concern to more mundane, but sociologically exemplary, topics, e.g., collaboration patterns, secrecy, and differential productivity. Doubtless, Zuckerman got her laureates to speak, but subordinated their words to the tune of "social structure and organization" in science. A recent rendition of this tune goes:

> [Scientific institutions and processes] have a logic and a structure of their own that interact with but are not determined by the cognitive contents of science and the organizations of research (Ben-David, 1977: 265, italics added).[8]

In Zuckerman's defense (and Ben-David's, for that matter), "selective reception" is an affliction which plagues us all; the fault lies in the remedies we may or may not seek. The strongest remedy is multiple indicators about which neither enough can be said nor too often. Multiple indicators may quiet the symptoms of selective reception, but the affliction lives on (see Gilbert, 1978:16).

For how, in the words of Mulkay (1974:110), "do we use these various kinds of partially conflicting information to reach a valid inference?" If we view validity as a range and not a fixed point, as we must, then commitment to multiple indicators will include the collection of protagonists' retrospective accounts of what they did--heard, spoke, and saw--"back then." This is an extra check which, at least, corrects the "misleading impressions of steady, undeviating advance toward the state of knowledge which now exists" (Mulkay, 1974:111). Further, it re-creates, within the bounds of recollection and self-justification, the chronology of actions and reactions which are rarely registered in even the most introspective scientific writing.[9] Interviewing helps, in short, to demythologize--to distinguish historical accuracy from scientific accuracy, "great men" from great acts.

Perhaps the best example of the interview as a demythologizing tool

involves the triad of co-discoverers, the Nobel laureates Temin and Baltimore plus the unsung NCI microbiologist John Bader who refused to stake a claim in 1969 based on his inhibitor data--data which prompted Green to postulate the existence of an RNA-directed DNA-polymerase (Green and Gerard, 1974:190). To rely solely on written accounts of the events culminating in the discovery, one would credit Temin with bold hypothesizing in 1964 and dogged determination in conducting six more years of experiments before demonstrating the plausibility of the hypothesis. Baltimore, while a newcomer to the research site, would share the credit for executing the crucial experiment, too. Bader today remains an obscure footnote in the history of the discovery. That his work paralleled Temin's is suggested once in the latter's Nobel address (Temin, 1976).

From our interviews, however, it would seem more than justified that the uncited, unfeted Bader share some of the recognition allotted Temin and Baltimore. Baltimore confesses that he "jumped the fence" for two days to do the experiment. The virus used was obtained by a phone call to his old friend and NCI project monitor George Todaro. Ironically, around this same time, Bader's request for virus from the Viral Oncology Program was declined because he wasn't a contractor doing research in that area. In Bader's (1978) words:

> I wasn't working in Viral Oncology at the time. I was working in Chemical Carcinogenesis. I applied twice for batches of virus to the National Cancer Institute, to Viral Oncology, and they said that it had all been committed to the Program, and I wasn't part of the Program.

Bader, ever the textbook scientist guided strictly by the data and shunning speculation, confessed "no regrets" for his behavior:

> I've always had the feeling that ideas were a dime a dozen. And for anybody to go around making conjectures and hypotheses, it may be interesting, but, really, that's blowing in the wind.

Our impression is that he might have been more assertive, but the "unimaginative" label hung on him by a few of our subjects seemed little more than ignorance of the man and his work.

Bader also pointed out that the first paper on the requirement for DNA synthesis in reproduction of RNA tumor viruses is his 1964 paper, published 5-6 months prior to Temin's statement of the provirus hypothesis. Written history attributes to Bader a 1965 paper (appearing in the same journal as the 1964 one, Virology) that builds on Temin's hypothesis. As for the criticism that data produced by inhibitor research is of questionable validity, Bader suggests, without rancor, that this is not a problem for a careful experimenter, but that Temin's experiments were sloppy. This may have accounted, in part, as other of our subjects conjectured, for Temin's departure from Cal Tech and the hubris attributed to him by his postdoctoral supervisor there, Harry Rubin.[10]

Thus, the Temin-Baltimore triumph is a far more intriguing story when told orally then pieced together from documents. It is more than a story of one man being anticipated (Bader), another moving patiently and inexorably toward the discovery (Temin), and a third independently duplicating the discovery, almost on a lark (Baltimore). The discovery of reverse transcriptase took at least 6 years and 3 diverse personalities to unfold. The retelling took--and yielded--much more.

Structural interviewing thus adds a dimension to the fathoming of fact from perception--this exercise of reconstruction--by socially constructing reality in the most basic sense. Since targets are selected primarily on structural grounds, it is their supposed divergence of perspective that is brought to bear on inference-making. The convergence of these perspectives is the measure of validity alluded to (as a range) above. Validity is not

absolute; it is rather an approximation, a socially-induced consensus wrought of intersubjectivity (Mulkay, 1978).

Reconciling inconsistencies in evidence by appealing to more and difference kinds may seem a nuisance, a needless muddying of the waters. Our reply to this contention echoes that of Mulkay (1974:118):

> . . . methodological theories either implicit or explicit . . ., underlie the use of every kind of evidence.

Inherent in every kind of evidence, in other words, is error which must be "controlled." Multiple indicators afford some control by freeing one from particular distortions or biases of data. Multiple indicators render theories of data explicit by acknowledging imperfections in measurement and impelling the investigator to seek additional data. Though structural interviewing is far from definitive, we believe it is part of an affirmative empirical approach to reconstructing the precise interplay between the cognitive and the social in science.

NOTES

[1]This has been the focus of a recent paper by Granovetter (1976) who, like us, is groping for a network sampling algorithm; his purpose and approach, however, differ radically from our own (also see Burt, 1977).

[2]The "pictures" were produced in our project by a multidimensional scaling routine which translated frequencies (or strengths) of pair-wise relationships into distances depicted in two dimensions (for details, see Chapter 6 and Appendix B).

[3]Structural sampling is somewhat akin to "dimensional sampling" (Arnold, 1970), a framework for drawing a small purposive sample representative of a population on n specified dimensions. The dimensions framing a structural sample are network positions and organizational ties, i.e., lab sites where authors perform, collaborate, compete, and cite.

[4]In two cases, it should be noted, the initial contact was made by phone in which the subject's secretary, and later, the subject himself, were informed of our intentions and arranged the meeting.

[5]In only one case did the subject continuously evade our policy queries by deflecting them into rambling technical expositions. Upon terminating this interview, we vowed, out of our dissatisfaction, to elicit more direct answers, and develop in subsequent interviews other subjects' perceptions of this remarkable man who was known to so many. His professional personality, as we later learned, tends toward long-winded, perceptive, and optimistic statements about cancer prevention programs. This subject had not duped us after all, but he knew far more than he shared with us.

[6]Still, researchers in foreign countries and on the west coast of the U. S. (particularly the Bishop group located at the University of California Medical School in San Francisco) had to be omitted from consideration due to the limitations noted earlier. Curricula vitae were requested from these 26 scientists, as they were from the subjects we did interview. Appendix C presents a demographic analysis of our interview subjects and mail respondents.

[7]Mulkay (1974:114) is quick to qualify this statement, admitting that, "As the research proceeded this division of labour became less distinct, so that I [Mulkay] contributed to the analysis of intellectual development and, more noteably, my partner contributed to the analysis of social processes."

[8]This independence of logic and structure from the "cognitive contents of science," of course, is what we have argued against throughout our study. The methodological point is that to assume such independence precludes the collection of a whole class of data, namely, that which relates to ideas and could be viewed as variables upon which social structure in science depends.

[9]Woolgar (1976b) has found, for example, that "discovery accounts," as developed in a series of interviews over 2-3 year periods, tend to be unreliable, i.e., subjects juxtapose, delete, and generally scramble

events. "The facts" assume a status that is less stable and authoritative than the historical record shows.

[10]Recall Rubin's skepticism about the discovery of reverse transcriptase, even after the enzyme activity was observed in a wide array of viruses (Spiegelman et al., 1970). The existence of the DNA provirus was finally confirmed to the virologist's satisfaction by Hill and Hillova's (1972) demonstration of infectious DNA for Rous sarcoma virus (Temin, 1976:1077-78).

THE POLITICS OF CANCER

NORMATIVE STRUCTURES AND FUNDING THE MISSION

> ...cancer is not simply an island waiting in isolation for a crash program to wipe it out. It is in no way comparable to a moonshot--to a Gemini or an Apollo program--which requires mainly the mobilization of money, men and facilities to put together in one imposing package the scientific knowledge we already possess.
>
> Instead, the problem of cancer--or rather the problem of the various cancers--represents a complex, multi-faceted challenge at least as perplexing as the problem of the various infectious diseases.... We have barely begun to perceive the fantastic array of causative factors involved in cancer, the methods by which they work, and the agencies by which they may be controlled. We are not yet ready to start a countdown for an anti-cancer blast-off, no matter what emotional appeal such an approach may have to the public.
>
> Philip R. Lee (in U.S. Senate, 1971a: 140-141)
> Chancellor,
> University of California

> Internal NCI affairs are very complex, and outsiders comment on them at their peril because there are so many political issues involved.
>
> Howard M. Temin (quoted in Wade, 1976:531)
> Nobel Laureate

INTRODUCTION

Policy is "essentially an ordering of priorities" (Greenberg, 1967:200). Policy research purports to contribute to the ordering. Science policy researchers who so choose can cast aside their naivete and plunge, as Temin suggests, into the world of biomedicine. The choices, however, do not end there. For the sociologist of science who simply seeks to understand the effects of biomedical research policy in the United States on the development of research

(perhaps hoping in the future to offer advice on the ordering or reordering of priorities), a second choice concerning "distance" must be made. How close does the sociologist dare to get to the politics or a dread disease? What kinds of information does one deem to be germane to an assessment of current and past priorities, outcomes, and mechanisms of control? How, in short, could the sociology of biology contribute to policy decisions?

Such choices must necessarily be made prior to the assessment or empirical study itself. The choices of sociologists of science, however, have been virtually unanimous: keep your distance and adopt a framework that is socially impeccable--attentive to organizational structures, institutionalization, and the ethos of science--but impervious to intellectual content. Another choice, one that sociologists have spurned or avoided altogether, namely, the use of the structure of argumentation of a field to scrutinize the efficacy of its policies and their relation to the social setting of new research priorities, seems to be more realistic and helpful for the scientist and policy maker.

But how do policy decisions for funding, for instance, affect this scientific argumentation process? Biological scientists, e.g., James Shannon (see epigraph, Chapter 1) seem to perceive the confluence structure of their problems and criticize funding policy from this perspective. What science would have been like had certain policies not been implemented, one will never know; but the sociologist can attempt to understand the political forces that shape the scientific problems that <u>are</u> studied. By understanding the political context the sociologist will be better able to understand the possible policy biases that are inherent in certain sociological techniques.

In later chapters (Chapters 5 and 6), for example, the problem of how self-citations within laboratories, laboratory size, and the density of networks interact will be discussed. If the size of laboratories is determined to a great extent by funding policy, then funding policy directly affects the perceived

social structure, measured by co-citations, as well. If, in turn, the density of a network were taken as an indicator of the "hotness" of a subject area, one might be greatly misled. What is perhaps even more to the point, if policy in turn were based on such perceptions (namely, more money invested in the large laboratories that represent dense clustering patterns in terms of co-citations), one would be involved in the self-confirmation of an inherent methodological bias.

The study of science policy is, of course, a subject in itself. The more limited goal here is to observe the critical discussion surrounding the early stages of the war on cancer. Specifically, we seek to raise sociological consciousness about the rhetoric and organization of science as they impinge on biomedical research policy, funding, and the disposition of knowledge.

THE CONTEXT OF ASSESSMENT

U.S. medical research policy, as was evident by the late 1940's "was not going to be established by hard consensus on a grand design. It would be fragmentary and incremental; in short, evolutionary" (Strickland, 1972:50). The evolution of the cancer research policy,[1] however, was interrupted by the passage of the National Cancer Act in 1971.[2] With the influx of monies into cancer-related research (Kalberer, 1975 and Figure 3.1) the evolutionary course of basic cell transformation research was to be profoundly altered.[3] The prospect of a National Cancer Authority, envisioned in Senate Bill S.34 as separate from NIH and possibly administratively independent of the National Cancer Institute, had generated scientific, organizational, and political cross-currents that were seldom found in earlier, small-scale biological research. As some (notably, Weinberg, 1965) had predicted, the time had come for Big Biology: in the past Big Physics met with success, so now the national funding focus must shift to the biological sciences.

The major contemporary impetus for expanding cancer research was heralded by former President Nixon's second State of the Union Message:

> I will also ask for an appropriation of an extra $100 million to launch an intensive campaign to find a cure for cancer, and I will ask later for whatever additional funds can effectively be used. The time has come in America when the same kind of concentrated effort that split the atom and took man to the moon should be turned toward conquering this dread disease. Let us make a total national commitment to achieve this goal (U.S. Senate, 1971a:74).

This request elicited an overwhelmingly positive response. Cancer as a symbolic threat was seemingly capable of unifying both Congress and the electorate (as Vietnam had not). In turn, it was difficult for scientists to find fault with this increased subsidy of biological science, given the broader context of slackening governmental support of science (Strickland, 1972:236-290). As Richard Rettig, in his thoroughly insightful Cancer Crusade: The Story of the National Cancer Act of 1971, states:

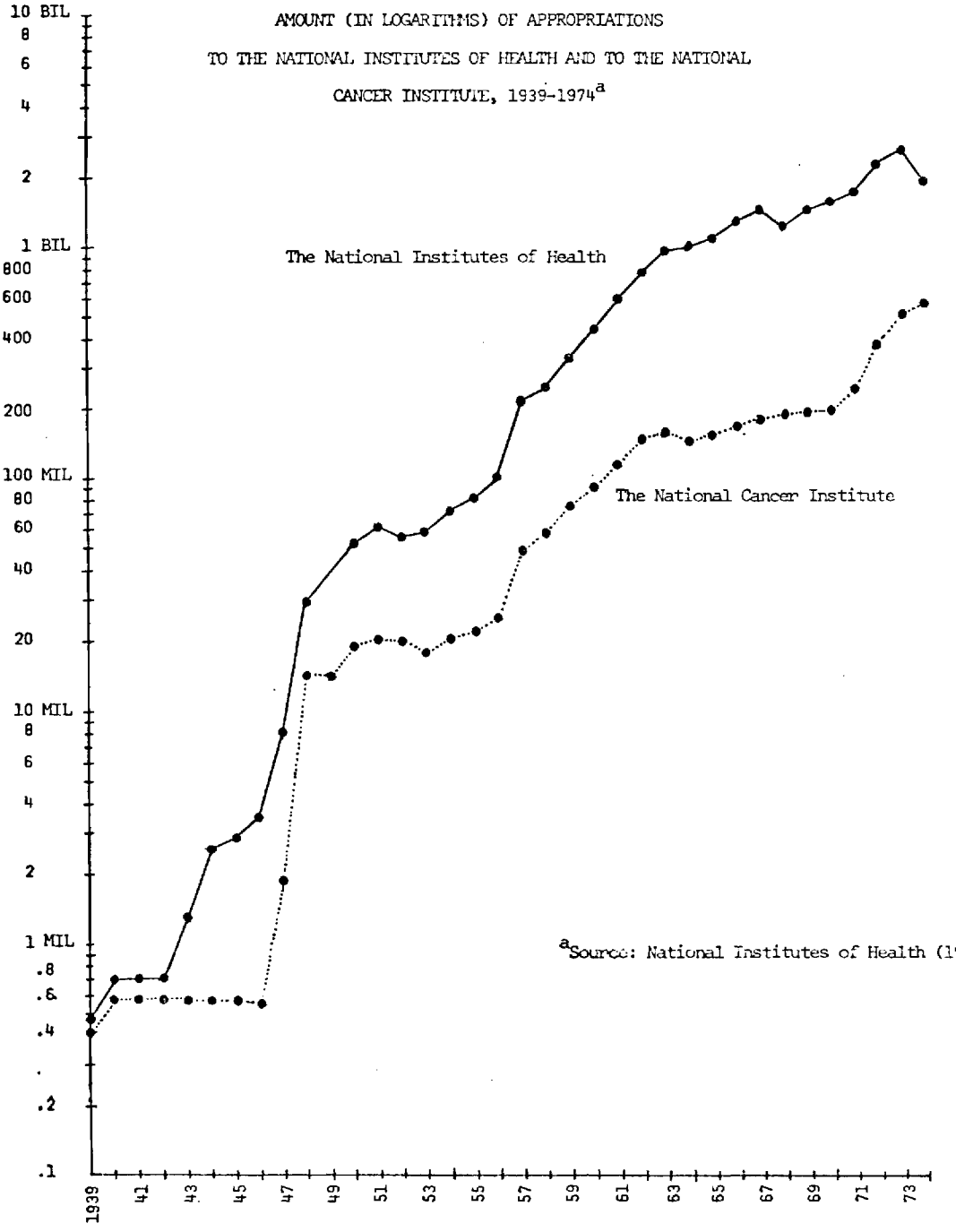
> In political terms, the Act is of interest because it indicates how a small but powerful elite composed of private citizens mobilized sufficient political resources to secure passage of legislation opposed by the National Institutes of Health and by most of the biomedical scientific community. In policy terms, the Act captures much of the current conflict between the public and its elected representatives eager to see life-saving and life-prolonging results flow from biomedical research and, on the other hand, a scientific community acutely conscious of the long time and great uncertainty characteristic of the process by which medical research is translated into clinically useful results (Rettig, 1977:xiii).

Predictably, some reservations about the funding of cancer research were expressed. Dr. Campbell Moses, medical director of the American Heart Association, noted before the Senate subcommittee on health,

> ...that if the state of research in cancer makes reasonable such a comprehensible effort to the control of cancer today, an exactly parallel effort is even more appropriate in the field of heart disease. In the cardiovascular field, we know that the expansion of our research effort, and the comprehensive full-scale application of the fruits of already available

FIGURE 3.1

AMOUNT (IN LOGARITHMS) OF APPROPRIATIONS TO THE NATIONAL INSTITUTES OF HEALTH AND TO THE NATIONAL CANCER INSTITUTE, 1939-1974[a]



[a]Source: National Institutes of Health (1975).

> research and technology, would save lives now (U.S. Senate, 1971a:231).

There resides in these remarks an attitude that the cardiovascular field (which studies the number one killer, heart disease) is ripe for research, riper in fact than cancer. Political symbolism and scientific investment priorities seemed, at least to some, confused. But while such reservations are implied, there was also an unmistakable endorsement of the principle of funding the cancer program. As Senator Edward Kennedy remarked at the close of this testimony, "There are those who say if you can't get a raise yourself, the best thing that can happen is for the fellow next to you to get a raise" (U.S. Senate, 1971a:232).[4]

So while most scientists were reluctant to criticize an allocation to the "fellow next to them"--there was tacit agreement that Big Biology was needed and cancer was a strategic site--reservations centered on research organization. How was Big Biology to be organized? It is customary in sociology when such views are expressed about organizational structures to think in terms of vested interest, power maintenance, or political gain; what emerged was researchers' concern about how the Cancer Act was to be administered and how this related to the development of biomedical science. Whether it be administered autonomously as a NASA-type special project or as part of the existing NIH organization was more an issue of research styles and reasoning processes within biomedicine than one of maintenance of role relations and political allegiances. If one takes at face value, at least provisionally, the scientists' expressed concerns, the issue at hand in the Cancer Act is how biomedicine should interface with existing organization.[5] Aside from the inevitable charges of favoritism and the abuse of power (both discussed below), the scientists' rhetoric centered on impediments to the advancement of knowledge inherent in the "mission mentality." What is at issue was the cognitive orientation toward research mobilization which the biomedical scientists shared, and which must be characterized if one is to understand their

critical posture toward organizational imperatives,[6] e.g., new funding mechanisms.

The biomedical researcher in the 1960's had already witnessed the pressures toward bigness and the scientific difficulties with mission-oriented research. As HEW Secretary John Gardner (1966:1602) cautioned in 1966:

> The question remains whether...we should mount large-scale, highly organized applied research or [other] developmental projects with specified short term goals. The answer [to both] is 'yes'. But in giving that answer we must bear in mind that each such effort is apt to be extremely expensive.... And we must not imagine that dollars and large-scale organization are an adequate substitute for ideas and a sound scientific base. Where the ideas and the scientific base do not exist, it is possible to waste vast amounts of money under the banner of practicality.

This caveat was echoed with even greater intensity by former (1955-1968) NIH Director Shannon in whose view, according to Strickland (1972:189),

> Targeted research, research aimed at finding cures for particular health problems, was...not only the most expensive but certainly the most wasteful kind. The waste was not limited to dollars, but included use of scientific energies, for research efforts narrowly aimed at single targets could restrict the beneficial effects of the internal dynamics of science. Moreoever, for NIH to place too much emphasis on directed research would be to retard the development of science in another way: it could artificially skew the production of new medical scientists.

Throughout these discussions it was the "scientific base," the "internal dynamics of science" that was being used as the rationale for organizational structure. Indeed, science was not to be equated with its organizational structure.

In the early 1970's, again many scientists recognized that the conquest of cancer was not a NASA-type operation because the problem to solve was essentially a nontechnical one. By early 1971 it was clear that the National Cancer Institute was destined to be reorganized in accordance with a new mission-oriented mandate. When Congress established the Special Virus Leukemia Program (1965) with an appropriation of $10 million, it became obvious what Congress had in mind. They wanted results! And to get results this Program engaged in more contract research than had been the custom of any NIH agency (see Kalberer,

1975:475f). Such contract research demanded, in turn, a new type of planning, and the operations research strategies that had worked for other areas of science were once again called upon for assistance. Director Carl G. Baker, Frank Rauscher, the newly appointed Chairman of the Special Virus Leukemia Program, and Louis M. Carrese, a systems management specialist set out to develop a rational basis for mission-oriented (therefore, contract-oriented) cancer research (Baker et al., 1966; Carrese and Baker, 1967; see Culliton, 1973). Although they clearly recognized the limits of organizational theory when it was applied to biomedical research programs, and although they desired to remain as flexible as possible, they were nevertheless constrained to make certain assumptions limiting the scope of the program: "The main assumption or working hypothesis on which the over-all program is based is that at least one virus is an indispensable element for the indication (directly or indirectly) of at least one kind of human leukemia (including lymphoma) and that the virus persists in the diseased individual" (Baker et al., 1966). The emphasis on the viral etiology of cancer, in particular the importance of RNA viruses, had been organizationally blessed.[7] Scientific "rewards" were now to be defined in terms of various lines of research which would begin with this assumption and progress toward the cancer cure. Contracts, it would seem, were often stigmatized by the academics both because they represented this new organizational mandate, and because they removed research from the independently motivated researcher. But in the 1970's when medical researchers began expressing their concerns about the National Cancer Act, the organizational die had long been cast.

To most scientists, however, the intellectual underpinnings of cancer research were radically different from the usual input-output model of purely technological programs. If the technological model works, it works because the "black box" interposed between the inputs and the outputs is well known or imminently

knowable through some directed effort. In the case of cancer research, the contours of the black box are sufficiently vague that it is difficult to decide where not to invest.

How then, are research investments made (in the guise of policy strategies)? Surely there is disdain, expressed in the testimony before the health subcommittee of John A. D. Cooper, president of the Association of American Medical Colleges,[8] for the nonscientists' view of how scientific answers materialize:

> An unfortunate misconception apparently is developing that the mere injection of additional federal cancer research funds will produce somehow an instant cure for cancer. Its equally misleading corollary is that the key to the conquest of cancer--one of the grimmest and most intractable groups of diseases that besets the world's scientists--lies in the managerial efficiency and the capacity of the medical-industrial complex (U.S. Senate, 1971a:391).

Other excerpts from his address reiterate the point:

> The Manhattan Project and the space program have been cited as successful precedents for the establishment of independent mission oriented agencies. However, ...harnessing the atom and the space program were largely technological challenges built upon a firm base of fundamental scientific knowledge. Their targets, though far away, were defined. In the Cancer Conquest Program the targets are diffuse, unseen and largely unknown (U.S. Senate, 1971a:102).

> There is no instant cure [for cancer]. And to imply that money can buy one is as unconscionable as it is to suggest that the key problem is managerial rather than scientific... But one fact is clear: The mere size of the national investment in cancer research is not the substantial determinant of speed in the conquest of cancer... (U.S. Senate, 1971a:392).

The stark contrast of the views of Benno Schmidt, who chaired the National Panel of Consultants on the Conquest of Cancer, forerunner of the existing President's Cancer Panel,[9] with those expressed above exemplifies that the organizational debate cuts deep:

> The valid analogy is not the scientific analogy but the organizational analogy. The cancer program, in order to succeed, needs

> the same independence in management, planning, budget presentation and assessment of program that those programs [splitting the atom or the space program] needed... (U.S. Senate, 1971a:196).

Rauscher (1978), recalling the passion of the debate, moderates Schmidt's tone:

> I think so long as the Plan is reflective of the state of the art and doesn't try to force the state of the art, and that was the big fear of course, that the bull's eye was going to be used to tell 'John' what he was going to do, and what he couldn't do, for that matter. It never was intended that way. I was a scientist, still am, and I would have rebelled myself at that. Carrese understands this very well, but many people don't understand that he understands it. And they still look upon planners now as a necessary, but almost undesirable, part of the picture.

It might just be that Big Biology demands organizational structures which, because of the "state of the art" and the nature of the discovery process(es), diverge from ordinary organizational principles in large-scale research. That is, sociologists may have too hastily dismissed the power, the needs, of scientific idea structures as formative agents of organizations. But does this not capture the sentiment of the biomedical community, i.e., give us Big Biology but not mission-oriented research. The problem seems to lie in the structuring of problems and intended solutions so that they escape the usual parameters of problem domain formation (see Chapters 1 and 2). The Nobel laureate and NCI critic, James Watson, asserts

> ...that high-quality cancer research is likely to be much more difficult to pull off than most other forms of biology.... [W]e may not have even one really hot clinical lead that has a good chance to lead somewhere soon with a major cancer. So we must be much more careful than we have in the past as to what we allow our lobbyist friends to claim for us.... We should do the science we are trained for and not hold the carrot too close.... But if we respond to the fear of less cancer money for next year by flashing out even shakier new leads, say, in tumor immunology, to mask the fact that we still have not made the big breakthrough, we have nowhere to go but down (quoted in Hixson, 1976:178-179).

On the one hand, such statements reflect the tremendous pressures on the biomedical community to effect a cure for cancer. This can be seen in the type

of question that the present Senate health subcommittee (still chaired by Edward Kennedy) continues to pose to the President's Biomedical Research Panel: "Why don't you people in the NIH and the medical schools spend less time 'understanding' disease and more time preventing or curing it?" (Culliton, 1976:33). On the other hand, Watson's statement reflects the peculiar nature of biomedical progress, namely, it is difficult to predict where a "breakthrough" will occur.[10] Finally, it reflects the "uneasy partnership" (Lyons, 1969) between government and science. The "fear of less cancer money for next year" elicits a public relations response from the scientist; this response must be that progress is being made.[11]

The separation of imposed social structures from the structure of biomedical progress seems to be the source of the scientists' concern. If this is the nexus of the policy problem for the biologist, then it would seem to be the most crucial site for sociologists to analyze. To assess the rationality of the biomedical research effort, the sociologist must question scientists' complicity with the organizational-managerial orientation to research criticized by Cooper. For cancer, of course, is much more than an area of scientific research; it is a highly visible symbol and thus peculiarly vulnerable to political abuse. The growth in the number of cancer victims, the shift in the age structure of the voting population and its possible partisan manipulation, growing concern over whether science could structure itself, and the need for conspicuous investments in science in the face of its dwindling public image (see Morison, 1969; Shils, 1972a; Toulmin, 1972) and proportion of the GNP, all capture something of the political tensions pervading cancer research. Cancer as a unifying symbol provides a basis for political mobilization that perhaps can be exceeded only by issues of national defense. Again a statement by Cooper is apropos:

> In an ideal world, the Association would say there is no need for new legislation to carry out a new scientific

> offensive against cancer. But the situation being what it is, there clearly is going to be some legislation (U.S. Senate, 1971a:393).

That "situation" was charged with political overtones, forces with which scientists were ill at ease. To compound the situation, House Bill H.R. 10681 was introduced.

The role of the author of this bill, Representative Paul G. Rogers (D-Florida), in effecting the compromise that the National Cancer Act of 1971 represented has been underplayed. Rogers paraded before his subcommittee member after member of the biomedical community who

> went on record in opposition to an autonomous cancer agency,... [supplying]...a refutation of the Senate [Kennedy] bill and justification for this [Rogers'] own.... The net effect was to suggest that the Panel of Consultants [led to Schmidt] supported only by the American Cancer Society [and Richard Nixon, as it were], was isolated from the mainstream of biomedical thinking (Rettig, 1977:233).

It was also a Rogers-called witness who drew attention to the discovery of reverse transcriptase by Temin and by Baltimore, noting (1) that this advance offered promise of determining whether viruses cause cancer in humans, and (2) that David Baltimore's work had been supported mainly by the National Institute of Allergy and Infectious Diseases. To Rogers, and undoubtedly his subcommittee, this basic scientist, who had unexpectedly contributed to cancer research from unrelated work, was an outstanding illustration of why the existing NIH should be preserved[12] (Rettig, 1977:235-236).

Although Congress was ultimately convinced of the necessity for continuing the new cancer program <u>within</u> the structure of NIH in the form of a compromise of S.34-cum-S.1828 and its House counterpart H.R. 10681, the <u>internal</u> politics of cancer remained tense. On 23 December 1971 as an invited guest described, the President

> came to sign the Cancer Act of 1971. Cancer research had entered the political arena. The Congressmen and Senators who guided the law into being smiled broadly as the cameras focuses on them. Most of the scientists in the audience did not smile; many were worried. The hoopla surrounding the Cancer Act implied the conquest on cancer in the near

> future because a couple of hundred million dollars a year more were to be channeled into cancer research. Those of us there who knew the 'state of the art' had cause to worry (quoted in Rettig, 1977:277).

Thus, political, organizational, and scientific components of cancer research signify all too well the alienation of the research process from the broader social milieu which supports it. Neither science nor society can afford to ignore the difficulties of mission-oriented Big Biology. When a Sloan-Kettering virologist, in the wake of the Summerlin "mouse-painting" affair, can say, "I have some advice for young researchers in biology. Stay out of cancer research because it's full of money and just about out of science" (quoted in Hixson, 1976:161), it is cause for concern. Whither the cancer mission?

THE MISSION AND CANCER RESEARCH ORGANIZATION

The demands of science, organization, and politics were and remain intimately intertwined in the campaign to conquer cancer. And the effectiveness of any evaluation of a program such as this demands that the various ingredients receive their due portion of credit and blame. As one might expect after more than seven years of the program, a lot of credit and blame is available. Daniel Greenberg has leveled severe criticism at the optimistic claims of finding a cancer cure that emanate from the National Cancer Institute, presumably seeing such claims as politically motivated and statistically suspect. The conservatism of the structure is also singled out for attack:

> My next visit was to the National Cancer Institute, where the official line is given on the record, but contrary views are offered only privately. 'It just doesn't serve to rock the boat,' a scientist told me. 'Look, when you've got 10,000 radiologists and millions of dollars' worth of equipment, you give radiation treatments, even if study after study shows that a lot of it does more harm than good. What else are they going to do? They're doing what they've been trained to do. Like surgeons. They're trained to cut, so they cut.' And research on prevention? 'It's picking up a bit after all these scare stories,' he said. 'But the level is actually

a joke' (Greenberg, 1975:4).

Has this conservatism of training, which for Greenberg includes a conservatism of treatment as well as beliefs regarding the etiology of cancer, stymied research progress? Even if it has not, Greenberg's alleged subordination of scientific knowledge to organizational structures suggested that the research tactics of NCI are often ill-founded and doomed.

Not surprisingly, there is a body of opinion, if not evidence (see below), to counter this charge. Many cognoscenti point proudly to the Virus Cancer (nee Special Virus Leukemia) Program. From our interviewees we learned:

> You can't mount a national effort when Dick Rauscher and John Maloney are sitting in their labs dispensing their own virus just to certain scientists. Great big technical problem. Can you produce viruses, virus preparations, of high potency, high infectivity, high purity, in vat quantities so that lots of people can work on them? Sure. That [Virus Cancer] Program did that.

> I think the NCI did a couple of things, and had to do some things, unlike the other Institutes. First of all, we had a number of line items in our budget which were, in effect, a mandate from the Congress to do so much at such a level in a particular field, not in a project now, but in groups of projects. Cancer control is a good example. Not only is there a separate appropriation for that part of the Program, but an authorization as well coming out of the Kennedy-Rogers committees. So part of what we did was in effect built in or locked in because of the mandates. We strove, and I think successfully, to maintain the emphasis and integrity of our basic laboratory structure within the Institute, but we did one thing different; and I still believe strongly that this is the way to go. We relied--we made a conscious decision to rely on--our good bench people in-house to help us manage outside programming. Management, scientific-wise, Carrese-wise, in my judgment was as important as doing good science.

> There was one key feature of the [Program] for which Rauscher and Maloney must take a lot of the credit. They foresaw, when the thing was going to expand, that there would be logistical problems in supply and demand. They knew there would be a requirement for viruses on a large scale, cell culture on a large scale, for animals, etc. So from the very beginning, they created a resource logistic mechanism which would solve that problem so that by the time that guys like me came into the field, and I said, 'Now, look,' I told them, for example, that I would isolate and purify reverse transcriptase for

> them within one year if they would give me enough virus, and I told them how much virus I needed. In fact, Joe Beard got his start because of my demand, and he started as a subcontractor under my contract. And when I went to Joe and I said, 'Joe, look, I need this and I'll give you the enzyme,' and I said, 'I need ten grams of virus about every two months,' he said, 'No problem,' and he delivered it. And we gave him the enzyme. We published the first purification in RT. That's the kind of theing that if the mechanism for underwriting Joe does not exist, if I couldn't go to the logistic department saying, 'Look, I need another 100 thousand bucks to do this job,' and them say, 'Okay, here it is,' that would have taken ten years to do, or maybe even twenty, if you have to use piddling amounts of virus. And the reason I was able within two weeks after confirming the thing with Rauscher [virus], we confirmed it with nine different viruses. The reason is that I had those viruses available to me.
>
> The Virus Cancer Program, because of its existence, and the level of funding that it had, or that it has, has provided a level of support for one relatively small area of science, that's far out of proportion to its total scientific impact. Now, its medical impact, you can argue about. I consider that the Virus Cancer Program was a good guess, because if you were going to try to hit the problem of cancer hard with an integrated program that you probably could not have made a better choice than to focus on viruses. Because the outcome, if you were right, if the guess was right, the outcome would be impressive. Yes, it was not as right as maybe we might have thought it was, and so the results have been not as dramatic. They've been scientifically very productive.

Such guarded praise of the Virus Cancer Program only hints at the evils it came to symbolize. It was perhaps the most visible contract program amidst the glaring visibility of the cancer mission, as operationalized by NCI. It was, therefore, the most vulnerable to criticism by the scientific community as well. And criticism--some say a surfeit of criticism--it surely got.

The Virus Cancer Program and Organizational Criticism: The Zinder Committee Report

In 1974, the report of the ad hoc Zinder Committee, so named for its chairman, Dr. Norton Zinder of Rockefeller University, was submitted to the National Cancer Advisory Board.[13] This committee had been constituted after growing criticism of the Virus Cancer Program (VCP) indicated that an evaluation--not unlike the evaluation of NIH by the Wooldridge committee a decade earlier--was in

order. As then-NCI director Rauscher (1978) told us:

> ...I appointed the Zinder Report, and I called for it. That was my thought. I'll never forget, I talked at coffee with Benno Schmidt, and I said, 'Benno, are we going to have to appoint a major group to come in and take a look at this from the outside?' On the way back into the room, we talked to Jim Watson and Jim thought it was a fine idea. Before that morning session was over, Jim had walked around my side and given me a slip of paper and given me names that he suggested, and I used most of those names, as a matter of fact, to the chagrin of many of my colleagues in virology.

The Committee's comments were generally harsh and pointed:

> First, the committee said, the VCP is too expensive. (It costs about $50 million to $60 million a year and consumes slightly more than 10 percent of the total NCI budget.) Second, the program must be opened up to the scientific community. At present, it is run by a handful of persons who have undue control over large amounts of money, which goes to only a limited number of laboratories. Furthermore, the individuals who award contracts are in a position to award them to each other, which somehow does not seem quite right. The committee called for new management practices and a good stiff measure of peer review by outside scientists (Culliton, 1974:143).

Centralization of (S)VCP funds was perhaps the most devastating finding of the Zinder report. A few individuals were found to dispense enormous sums of money annually, notably $19 million to Robert Huebner (Chief, Viral Carcinogenesis Branch, NCI), $7 million to George Todaro (Chief, Viral Leukemia and Lymphoma Branch, NCI), and $12 million to Robert Manaker (Chief, Viral Biology Branch, NCI).[14]

> It was only natural that when the SVCP was formed [initially to explore the possible role of adenoviruses in malignancy], a small group of investigators was involved--an 'in group.' It now represents a somewhat larger 'in group' of investigators. Administratively its procedures lack vigor, are apparently attuned to the benefit of staff personnel and are full of conflicts of interest. Because the direct targets have become fuzzy since 1964, although the available funds have continued to grow, the program seems to have become an end in itself, its existence justifying its further existence. In doing so, it is eroding what is good in both the grant and contract mechanisms, a fact which may account for the

widespread antipathy to SVCP in the scientific community (quoted in Hixson, 1976:132).

A non-NCI researcher informed us:

> I was on the National Cancer Advisory Board when the Zinder report was proposed, and I was one of those who asked for it because there were clear deficiencies in the VCP program. There were other weaknesses, because it grew up too fast and things were done that probably would have been done a little bit more rationally, and there were areas of abuse, which should be corrected, and that's essentially what the Zinder report tried to do. And I don't think there was any serious question in the minds of the Zinder Committee, and it certainly didn't appear in their report, that the science that was being done by the VC people was excellent, and there was no doubt that of all programs that were going on at NCI, as well as the clinical ones, it was the one that was producing the new science for the buck. And so all in all the thrust that was in the report was to correct obvious undesirable features of the VCP. [Administrative?] Administrative, and conflicts of interests which existed between in-house people and out-house research (I use the word out-house advisedly)--and so on and so on. I think that's perfectly okay. The Zinder report was a good one and it served a useful purpose and to a large extent was implemented. It took a lot of time because you had to tread on some very powerful toes to get it done, so it took awhile. But Rauscher started to move in that direction and Maloney certainly did, and a lot of wings were clipped, not completely--I mean, those things are very difficult to reverse.

Rauscher (1978), in defending the Program, observes that

> When we first got that ten million dollars in 1966, the Congress said, 'You can have the money, but you've got to spend it by contract.' That was a mandate, too, incidentally. We had no choice. We couldn't go grants if we had wanted to.... And at that time we used some of the money in order to get off the ground very quickly through a sole source contract, some of which was to support what I thought were exceedingly good ideas by in-house investigators. Well, as the dollars became bigger and as these fellows became successful in the sense of publishing many papers, they began to be resented by people on the outside who had to compete for every dollar they had--again, totally predictable.[15] So, yes, when you say that the second criticism was one of administrative arrangement, or of the technique of management, you are absolutely right.

Rauscher also impressed upon us:

> Number one, during all of that Program, or much of that Program, you could not grant outside of the United States, but you could use the contract mechanism, and a major focus was opportunities

abroad, so they had to use the contract mechanism. Number two, you can not grant to a commercial organization. You must use contracts, by law and regulation. Number three, many of our best scientists in this country happen to be in commercial organizations and you want to do work with the best scientists. And that was, I think, overlooked [by critics].... I was there when Mr. Nixon flew out of the sky and said, 'You now have Fort Detrick.' Not many of us wanted it. I think it was wise to take it, not only for cancer, but for the rest of NIH, as I thought at the time. But, in effect, in getting no more money to take over Detrick, he was saying, 'We're going to turn swords into plowshares for cancer research; for the public of this nation, you run that facility.' We were not given any positions to run it, and we recognized immediately that we would have to contract with somebody to do it--the way Union Carbide runs Oak Ridge, as an--incidentally, that was the model we used. And, again, I think wisely so. So in effect, right now we're locked in. We, meaning the NCI, is locked into something like 25 to 30 million dollars a year to keep a very good facility with very good people going, but it does not have the same peer review because it can not, for individual projects, within that 25 million dollars. It's almost like NCI. My people at NCI got their support as a cut off the top. You know, they had to write an annual report and so forth, but in effect their support was guaranteed. They didn't have to write a grant request, contract request, or what have you. Neither do the individual scientists at Litton.[16] When they compete, they compete for...a three year kind of project...and for that period of time, they're pretty safe.

Finally, another academic researcher offered this view:

The difficulty that the Zinder report was talking about was the prime commercial laboratories that were getting fortunes to do very little, and they seem to have been too well entrenched to have been touched, at least under the previous administration at NCI. And there you have to look very closely at the power of a single man [Maloney?]. Without a doubt. And he determined what was being done and he would go up and hire a position, and if he didn't get payoffs from all these people, then he certainly got payoffs in terms of friendship and self-importance. And that was, for some reason, untouchable. And that grated on our scientists' self-image and pride, because it was such poor work being funded, such an enormous amount of it, that that gave the VCP a terrible name for producing the worst kind of science at the most cost. But in fact the VCP also supported some of the very best work around the country. And had they been hard-nosed about what they supported, and had they insisted on the principle that you only support good science in the best places doing that kind of science, instead of saying, 'Well, we have all this money and we have to spend it somehow, and we've got all these guys who think they know what they're doing, so we'll give them the money and let them work,' then they would be in much

better shape now.

The administrative upshot of the Zinder Committee review was, in Rettig's (1977:301) words, an

> 'opening up' the VCP program specifically through the establishment of an NCAB oversight committee, a reconstitution of the contract review groups, and the elimination of contract work that was an extension of VCP scientists' intramural research. The NCAB did establish a subcommittee, chaired by Dr. Harold Amos of Harvard Medical School, to monitor the program's response to the Zinder Committee's recommendations. The VCP, on its part, has established an advisory committee of non-program scientists to provide advice on broad directions of resource allocation, promising lines of scientific inquiry, and means of application of research findings. The contract review process was also modified to increase the rigor of review of individual contract proposals. The Amos subcommittee, in its report to the NCAB of June 1975, indicated its general approval of the program changes.

In addition, NCI has sought to streamline and standardize its procedures for reviewing contract and grant proposals alike (see below) though many of our interviewees doubted the effectiveness, or even the sincerity, of the announced (see Rauscher, 1974) modifications.[17]

The Zinder Committee also concluded, however, that "about 50 percent of the program is supportable at some level" (Culliton, 1974:144). This criticism is partially based, like that of Greenberg quoted earlier, on <u>intellectual</u> investments of scientific administrators and researchers that are organizationally and politically expedient, but reproachable on grounds of knowledge (or ignorance):

> Many of those in administrative control of the VCP are men whose careers are intimately linked to the idea that there is a relationship between certain RNA viruses and human cancer. Much of the research the program supports is aimed at substantiating this idea. VCP support of research on DNA viruses is comparatively small. The committee recommends...an integrated program with a built-in series of checks and balances to prevent the special notions of particular individuals from carrying the day. For example, should the first definitive [human] cancer virus turn out to be a papova virus [one of many suspected DNA viruses], the VCP would be in a strange position. It scarcely supports any work in this area and only recently has gotten seriously involved with the DNA viruses such as herpes (Culliton, 1974:144).

How then, should such "internal" criticisms be utilized by the sociologist of science? The criticisms certainly provide a baseline for interpretive evaluations of research in progress which sociologists can then approach from other perspectives; in short, they help generate hypotheses. But there would appear to be an even more central role for criticism that incorporates the scientists' evaluations of organizational principles and practices. Critical comments on organizational structures mirror evaluations of the proper workings of science; they are statements about the state of science and the norms of reason. These norms of science have a function within the knowledge component of science. Unlike the traditional conception of the norms of science (Merton, 1942; Barber, 1952), which supplants the knowledge function with an organizational maintenance function, a formulation is needed which restores to centrality the cognitive orientation of researchers. It should take into account the scientist's vocation as it becomes manifest both in knowledge and in organizational criticism.

In short, a single set of institutional norms no longer suffices as behavioral guides in the context of Big Science. What other norms have scientists embraced? Some answers have already surfaced in our examination of the cancer war and one of its most potent programs. Now we must pause to analyze systematically these normative manifestations of Big Biology. Then we shall return to the question of sponsorship of modern biomedical research at the institute level, i.e., how did the cancer mission affect allocations to NIH institutes other than NCI? After such analysis, the debate over the viral etiology of cancer may appear less idiosyncratic in (a) the funding priorities of research on dread disease and (b) the discovery process within biomedicine.

Interpreting Criticism: In Search of New Norms?

From 1971 to the present, the largest single biological research offensive that the U.S. has known has been directed by the National Cancer Institute. That the

funding of this institute relative to others within NIH has been the target of criticism flows not merely from disparities in allocations, but from disparate interpretations of the ethos underlying those allocations plus the rhetoric of the scientific community to secure them and "ensure" the advancement of scientific knowledge. To reflect on this rhetoric is to speculate on its antecedents and to recognize its implications for alternative normative structures in the community. To quote Nelkin (1975a:21-22):

> Merton's formulation [of the scientific ethos] was developed to reaffirm the values of science when it was faced with 'frontal assaults on its autonomy,' but assaults on science and its accepted values have become more vigorous, stimulated by growing perceptions of the importance of science to society and of its social consequences...The scientific community is ill-equipped to deal with external pressures. The norms of science [may] govern the behavior of scientists within their field, as if science were by definition an autonomous enterprise. But unlike physicians, or those in professional practice, scientists share no well formulated set of norms to govern their relationship outside the scientific community. The instinct to protect professional autonomy is backed by few rules that would guide an appropriate collective response. Thus, when unable to ignore persistent challenge, scientists often take refuge in reasserting the neutral character of their work and the irrelevance of political and social considerations.

This, too, according to Mulkay (1976), is a rhetorical device to loose upon unknowing nonscientists. Yet the ideological outworkings of such a stance are there for all to observe, including other scientists. Some observers, notably Mitroff (1974), have seized upon the ambivalence of scientists--which Merton (1965) saw as endemic to the institution--to demonstrate the counternormative behavior of individual scientists. These scientists see "interestedness" and perhaps non-"communality" (i.e., secretiveness) as <u>rational</u> responses to the hostility and incessant pressures which controversial research fosters, whether it be analyses of moon specimens which are taken to support pet theories about the origin of the moon, or commitment to a viral etiology explanation of cancer.

Both of these examples entail expensive, nationally visible and funded (NASA

and NCI, respectively) research. Both underscore what Orlans (1975) calls the "indiscriminate advocacy of knowledge" and Salomon (1972) terms "the mating of knowledge and power." Not only are scientists playing multiple roles of principal investigator, peer reviewer and science advisor, but they are instrumental in the disposition of knowledge, i.e., as advocate, popularizer and mediator vis-a-vis lay publics. Is it no wonder, then, that ambivalence arises from ambiguities regarding the cognitive and pragmatic dimensions of science? There is no consensus within the scientific community on these dimensions, yet in dealing with non-scientific publics, near unanimity must often be sounded if research efforts are to be sustained. As Nelkin (1975a:26) suggests,

> ...scientists engaged in research in policy-relevant areas may select research questions that are based less on disinterested judgments of intrinsic scientific merit than on organizational imperatives of their institution, or on their perceptions of social utility.

To recapitulate, what Merton characterized as the scientific ethos, the universal cultural values of the scientific community, can now be seen as a class of stereotypical, ideal, institutional norms. Their technical counterpart, in the sense of more transitory research-specific content (theory and method) which constrains practitioners and guides the evaluation of knowledge-claims is what Mulkay (1969) labeled cognitive norms. These norms may encompass counternormative behavior and all the ambivalence that scientists may experience over a particular research problem at a particular time. A third class of norms, however, we may term rhetorical since they provide

> vocabularies of justification, which are used to evaluate, justify and describe the professional actions of scientists, but which are not institutionalized within the scientific community in such a way that general conformity is maintained[18] (Mulkay, 1976:654).

The import of this third class stems from scientists' selective presentations of views to support their collective research interests. Rhetorical norms, therefore,

govern the articulation of an occupational ideology to non-scientific, but powerful, publics such as government agencies and congressional committees.[19]

With these classes of norms in mind, we can return to the original context for this discussion and reassess the assertion that the mission orientation of contemporary biomedicine, especially cancer research, seems to demand a new type of research organization. What has been the funding structure that propels cancer research and how does it compare with other research (i.e., institutes) within NIH?

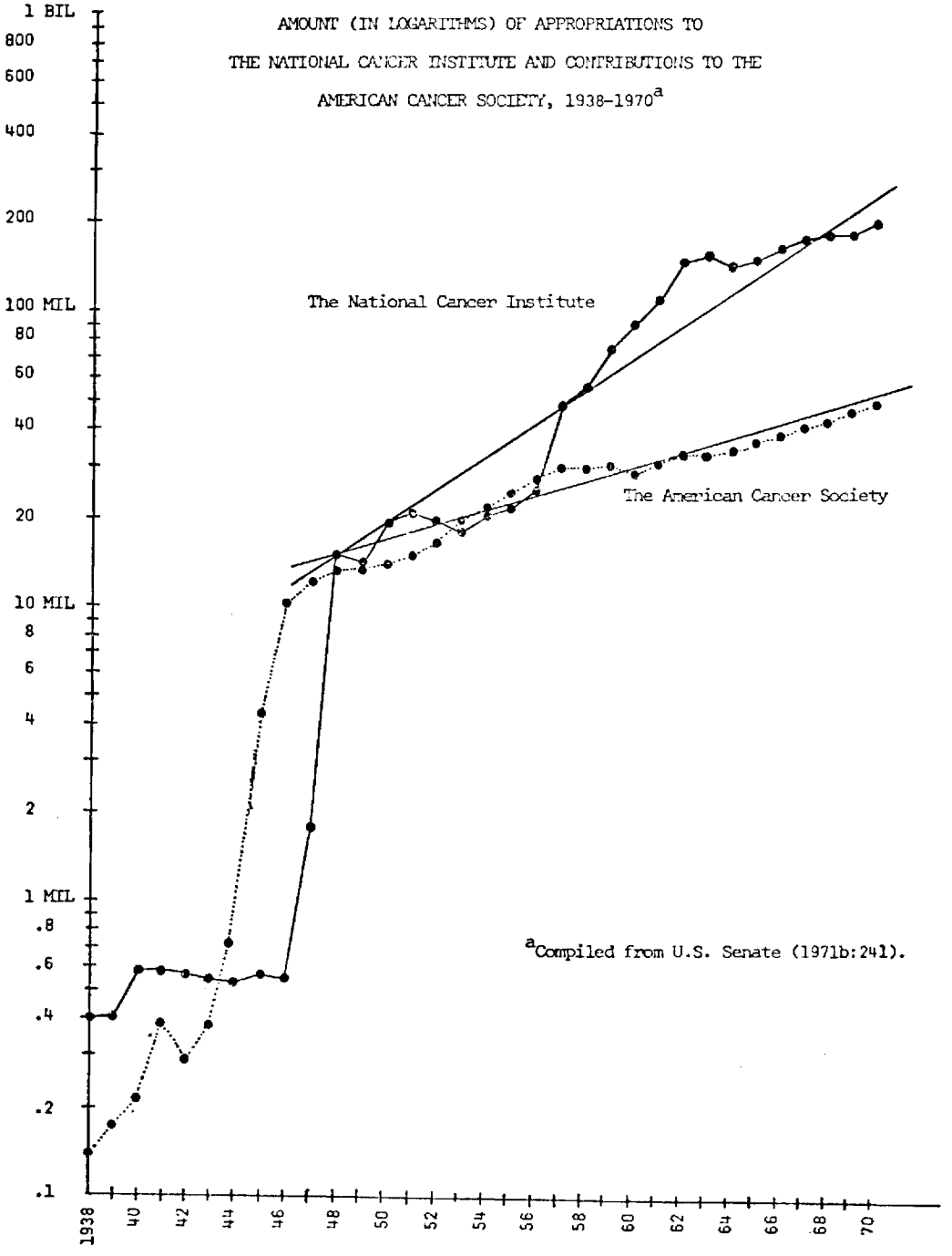
NCI AND FUNDING PROSPERITY[20]

The two major sources of funds in the U.S. for the support of cancer research have been the American Cancer Society (ACS) and the National Cancer Institute (NCI). The smooth increase of the ACS curve since 1948 (see Figure 3.2) is no doubt indicative of a set of public beliefs about cancer and its cure.[21] Numerous polls have demonstrated (e.g., Rauscher, 1974) that cancer is the most feared of diseases in the U.S.; it is therefore easy to imagine that this fear has been translated into concrete contributions to ACS.[22]

Comparing the amounts of ACS and NCI monies in Figure 3.2 lends credence to the claim that interpreting shifts in funding is perilous. Had cancer research reached a critical "take-off" threshold of knowledge in 1958 due to the cumulative effects of ACS and NCI funding prior to that year? Was the funding merely part of the overall increase of governmental interest in science stimulated by the Russian Sputnik? Did it stem from the general shift in attitude toward governmental patronage of science in the post-World War II era? Or had the public displaced the responsibility of biomedical progress from private organizations and piecemeal contributions to the federal government and its massive fiscal capability?[23] All of these explanations are somewhat plausible and the acceptance

FIGURE 3.2

AMOUNT (IN LOGARITHMS) OF APPROPRIATIONS TO THE NATIONAL CANCER INSTITUTE AND CONTRIBUTIONS TO THE AMERICAN CANCER SOCIETY, 1938-1970[a]



[a]Compiled from U.S. Senate (1971b:241).

of one does not preclude the validity of another.

But the steady gain in ACS contributions can be contrasted with the irregularities of the NCI curve. Kenneth Endicott (1969:2), former director of NCI, has argued that the major perturbations are due to the Korean and Vietnamese wars, respectively. This appeal to the conflict of funding interests and the need for federal allocation priorities in times of national mobilization is intriguing even though it appears obvious. Clearly, one is accustomed to such historical macro-level explanations when major external political clashes of nations are at issue. But when it comes to major internal shifts in priorities, e.g., wars on poverty, cancer, etc., allegations and debates about the impact of funding displacements ensue. With the passage of the Cancer Act in 1971,

> ...though separate agency status was not secured, substantial autonomy for NCI was obtained. The new law gave NCI a renewed and expanded mandate, raised the formal status of its director, provided the expectation for vastly increased funds, and in the budgetary by-pass mechanism established a procedure for asserting autonomy. The resolution of the legislative debate left the cancer crusade advocates with much of what they wanted, but gave the opponents the symbolic and material accomplishment of defeating the proposed separate agency recommendation (Rettig, 1977:291).

Although the "separate and unequal" doctrine was defeated by the Act, NCI had become the "I" in NIH. Querying Frank Rauscher about this privileged status, we heard a straining for consistency (that was not very convincing):

> Interviewer: Does it matter that the Cancer Institute is inside the NIH fold or outside the NIH fold?
>
> Rauscher: Oh, I always thought it should be inside.
>
> Int: But you were a minority on that.
>
> Rauscher: Yeah, I was not very popular when I said that, among some of the outside lobbying groups and pressure people. I was always firmly convinced that in order for the Cancer Program to be healthy, we needed a healthy NIH. We're part of a family. I wanted our people to be able to collaborate with somebody over in Allergy, or Dental, or what have you, and I think that was the right way to go. It could have been chaos, but it wasn't.

On the record, Rauscher has maintained that the decrease in funding of other institutes of NIH had little to do with the national mobilization against cancer:

> These reductions cannot in fairness be attributed to the existence of the National Cancer Program, although this would be difficult to prove beyond a doubt. On the other hand, it is just as difficult to prove that the other institutes would have received more funds if the National Cancer Program did not exist. In fact, I am told by people in the Office of Management and Budget that the latter would not have happened in 1972 to 1974 (Rauscher, 1975:118).

Off the record, however, there is some wavering:

> You know, when I was there [at NCI] I was a member of the Executive Branch, appointed directly by the President, and there are some restrictions under that situation. I didn't experience too many though, thank God. I was fortunate enough to have people like Mary [Lasker] and Benno Schmidt,[24] in particular, and direct access to the White House, so that when I went to a Congressional hearing and they said, 'But couldn't you use more money,' I would be able to say, 'Absolutely, my Board and Panel tell me we can use another 185 million.' And, I guess, almost no other Director of any other Institute had that option.

Elsewhere in our conversation Rauscher remarked:

> You have got to remember the first two years--the critical first two years--of major budgetary increases, that money did not come out of HEW funds. It came out of a pocket that Mr. Nixon had in OMB. So there was no way that we were competing for funds out of the HEW pocket. Very few people understand that point. In later years, that was not true. We were competing for what money was available in HEW.

In other words, the conquest of cancer was not entertained publicly as a viable explanation of concomitant decreases in funding for the other institutes. What were "obvious" macro-level explanations in fundings fluctuations when military mobilization was at issue are not invoked when the macro-level mobilization against cancer is raised. Privately, Rauscher is more sanguine--and less persuasive. His explanations differ in their intuitive (rhetorical?) appeal depending upon where, when, and to whom he is speaking. His is essentially a non-zero sum interpretation: "We won but you didn't lose." Yet it is plausible if allocations to NCI are seen as <u>independent</u> of the total NIH budget. To do so deflects interpretation away from questions of internal restructuring[25] (i.e., contracts vs. grants) due to programmed missions and increased allocations.

Even if the funding of NCI and of the rest of NIH are totally independent events, one must still deal with the whole picture of biomedical organization and support in the U.S. Adopting a critical perspective on the interaction of biomedical policy and its organization within the development of biomedicine, one finds that disproportionate funding of the institutes leads to "relative deprivation" which in turn elicits wholesale criticism of the total biomedical funding policy. Clearly, attitudes toward funding policy cannot be reduced to comparisons of the relative funding levels of the various institutes. Some comparisons, however, are instructive. Even though there was a fairly stable number of grants reviewed during the late 1960's and the approval rate was increasing slightly, the actual award rate was decreasing dramatically. This condition was created by changes in the rate of increase of available funds, shifts in the proportion of renewals to new grants, and the increase in the average cost of research grants. A tense situation was brewing <u>within</u> all

of the institutes. Kalberer (1975:478-479) observes that:

> In the case of NCI, award rates started to fall off dramatically beginning in 1968 as a result of the leveling off of Congressional appropriations. Consequently, there was a steady downward trend in the percentage of traditional grants awarded, particularly new grants, in 1970 the Institute reached its all time low level, awarding only 30% of approved grants. With passage of the Act of 1971 this trend was reversed. Within the last 4 years, at least 50% of all approved new applications have been awarded. [Clearly] the halcyon years, prior to 1964, when NIH, including NCI was able to fund more than 90% of all approved applications, have passed.

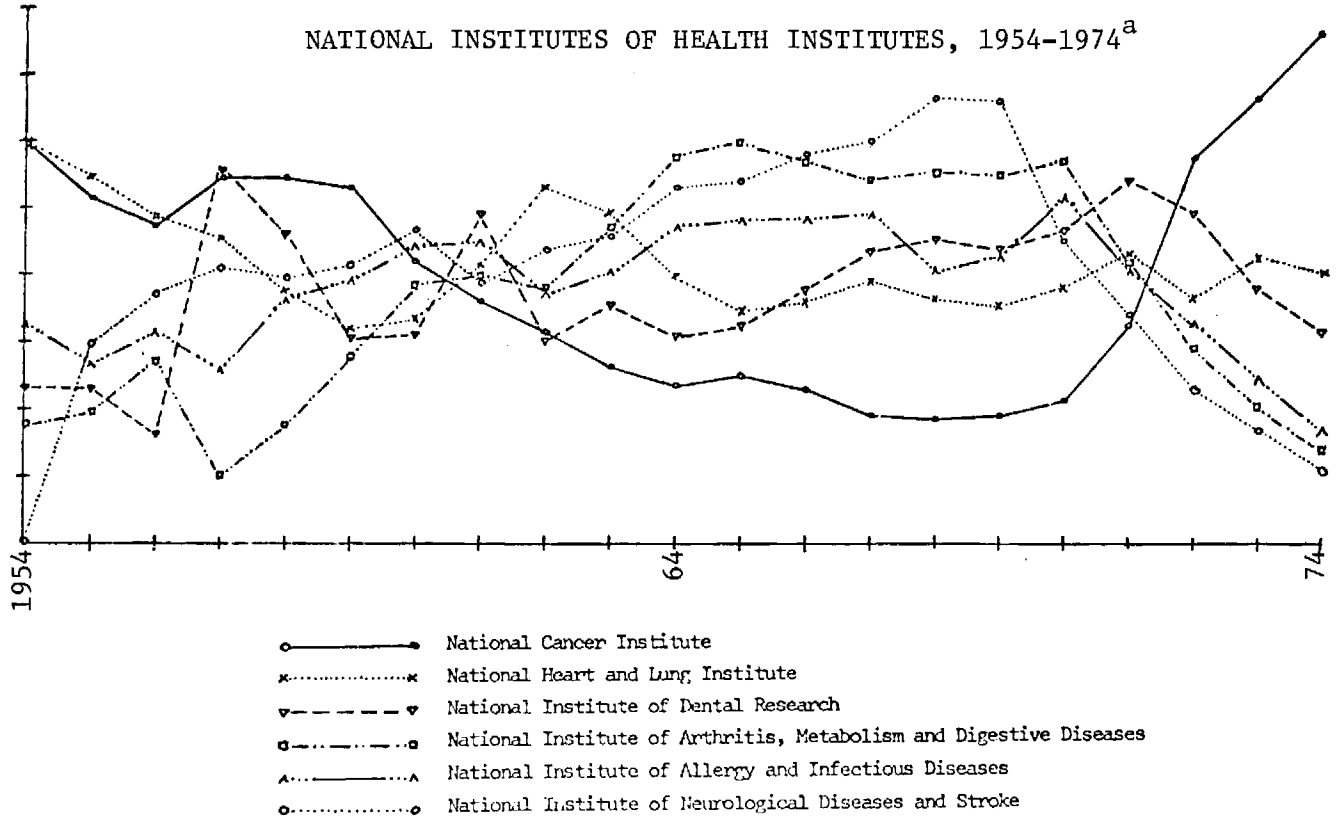
Relative deprivation could be perceived, thus, not only between institutes but also within institutes as funding capabilities changed in the post-war era. The new wars on cancer and heart disease did not occur in a vacuum; the basis for a feeling of deprivation was already present. What made the situation intolerable to many, however, was that they were being deprived more than others. It is this phenomenon of relative deprivation which Rauscher (1975:118) was trying to defuse. But even while Rauscher proposed the independence of the NIH and NCI budgets, Kalberer (1975:477) notes that "As a result of the decrease in budgets of the other institutes, NCI is making every effort to fund outstanding applications assigned initially to other institutes but not payable because of lack of funds." Recognition of the interdependence of the institutes seemed to obtain more in practice than in administrative theory.

To illustrate the problem of relative deprivation, Figure 3.3 has been constructed from the raw data reported in Table 3.1. In this figure the proportion of total NIH allocations received by an institute (the six major NIH institutes which have been in operation since at least 1954 are represented) for a given year is expressed as a proportion of the total NIH allocations it received during the entire twenty year period. Hence,

$$y = 100 \times \frac{\text{proportion of allocation in a year}}{\text{proportion of allocation over total 20 year span (1954-1974).}}$$

FIGURE 3.3

CHANGES IN RELATIVE PROPORTIONS OF FUNDING OF SIX NATIONAL INSTITUTES OF HEALTH INSTITUTES, 1954-1974[a]



[a]Compiled from National Institutes of Health (1975).

TABLE 3.1

ANNUAL APPROPRIATIONS WITH PERCENTAGE ANNUAL INCREASE FOR SIX INSTITUTES OF THE NATIONAL INSTITUTES OF HEALTH, 1954-1974[a]

Years	Institutes NCI	% Δ	NHLI	% Δ	NIDR	% Δ	NIAMDD	% Δ	NIAID	% Δ	NINDS	% Δ
1954	20.237		15.168		1.740		7.000		5.738		4.500	
1955	21.737	7.41	16.668	9.89	1.990	14.37	8.270	18.14	6.180	7.70	7.600	68.89
1956	24.978	14.91	18.898	13.38	2.176	9.35	10.840	31.08	7.775	25.81	9.861	29.75
1957	48.432	93.90	33.396	76.72	6.026	176.93	15.885	46.54	13.299	71.05	18.650	89.13
1958	56.402	16.46	35.936	7.61	6.430	6.70	20.385	28.33	17.400	30.84	21.387	14.68
1959	75.268	33.45	45.613	26.93	7.420	15.40	31.215	53.13	24.071	38.34	29.403	37.48
1960	91.257	21.24	62.237	36.45	10.019	35.03	46.862	50.13	34.054	41.47	41.487	41.10
1961	110.300	20.87	86.900	39.63	15.500	54.71	61.200	30.60	44.000	29.21	49.600	19.56
1962	142.836	29.50	131.912	51.80	17.340	11.87	81.831	33.71	55.341	25.78	70.812	42.77
1963	155.742	9.04	147.398	11.74	21.199	22.25	103.388	26.34	66.142	19.52	83.506	17.93
1964	143.194	-8.06	127.423	-13.55	19.166	-9.59	107.699	4.17	67.117	1.47	84.471	1.16
1965	150.011	4.76	124.824	-2.04	20.083	4.78	113.050	4.97	69.847	4.07	87.821	3.97
1966	163.768	9.17	141.462	13.33	23.677	17.90	123.203	8.98	77.987	11.65	101.153	15.18
1967	175.656	7.26	164.770	16.48	28.308	19.56	135.687	10.13	90.670	16.26	116.296	14.97
1968	183.356	4.38	167.954	1.93	30.307	7.06	143.954	6.09	94.442	4.16	128.633	10.61
1969	185.149	.98	166.928	-.61	29.984	-1.07	143.888	-.04	96.840	2.54	128.934	.23
1970	190.486	2.88	171.378	2.67	30.809	2.75	146.619	1.90	103.695	7.08	107.365	-16.73
1971	233.160	22.40	194.925	13.74	35.440	15.03	137.986	-5.89	102.368	-1.28	103.502	-3.60
1972	378.794	62.46	232.627	19.34	43.388	22.43	153.337	11.13	109.118	6.59	116.731	12.78
1973	492.205	29.94	300.000	28.96	46.991	8.30	167.316	9.12	113.414	3.94	130.672	11.94
1974	551.191	11.98	302.915	.97	45.565	-3.03	159.447	-4.70	114.000	.36	125.000	-4.34

[a]Compiled from National Institutes of Health (1975).

This transformation yields a good indicator of systematic changes in funding priorities within the NIH institute structure. In a completely stable situation, the proportions of funds would remain constant for the entire period, i.e., a line (y) would not deviate from 100 in Figure 3.3. Because of numerous alterations in the relative proportions of allocations within NIH, however, relative gains and declines in funding position are revealed that signal possible changes in the relative importance of research areas, political climates, etc., of the individual institutes.

Reflection on Figure 3.3 must be prefaced with some observations and caveats. First, it must be noted that several of the institutes did not come into existence until 1954. The origin years of the six are as follows: National Cancer Institute (NCI), 1937; National Heart and Lung Institute (NHLI) and National Institute of Dental Research (NIDR), 1950; National Institute of Allergy and Infectious Diseases (NIAID), National Institute of Arthritis, Metabolism, and Digestive Diseases (NIAMDD), and National Institute of Neurological Diseases and Stroke (NINDS), 1954. That NCI and NHLI enjoyed a comparatively favorable position in 1965 is probably due largely to the fact that they have been in existence for a period, are therefore well-entrenched, and have acquired funding momentum.[26] The three institutes founded in 1954 are, in this respect, at a decided disadvantage, since they are still searching for and developing new programs. In this light, the low position of NINDS, for example, signifies its low priority and status within NIH in 1964.

Second, we recognize that change of relative position may derive from several types of organizational shifts. An example is the rapid ascent of NIDR in 1957 fostered by an unusually large appropriations increase (177 percent, Table 3.1). In contrast, the precipitous "descent" of NINDS in 1970 is no doubt a result of the creation of the National Eye Institute (1968), a spin-off from NINDS (with independent budget status in 1970). Interpretations thus

must be tempered by an historical understanding of the development of the institutes; indeed, one should be encouraged that the transformation in Figure 3.3 mirrors the alterations of policy which we have already noted.

Our primary concern, of course, is the realignment of the institute structure in the 1970's, for this is the period of disjunctive, selective funding shifts. The stability observed from 1964 to 1969 dissipates markedly in the 70's. Although some of this may be accounted for, as has been seen, by the reorganization and separation of new institutes from established ones, the general tendencies seem to follow economic exigencies, new orientations to basic and applied research, and new policy decisions based on these views. From 1970 forward, the allocations of funds to NCI, NHLI, and NIDR (again consult Table 3.1) stand in stark contrast to the allocations of funds to NIAMDD, NIAID, and NINDS. The patterns of percent increase per year are striking in this period; the concentration of funds, especially in NCI but also in NHLI and NIDR, greatly depresses the relative positions of the remaining groups.

The new policy decisions which are implied by Figure 3.3 are the National Caries Program (1971) and the National Cancer Act (1971). These were both mostly mission-oriented programs designed to eliminate widespread afflictions. They were intended to produce visible results just as the intensified efforts of NASA had accomplished their targeted tasks. Institutes geared to less dreaded or universal diseases seemed to fare the worst after installation of the new policy. As was clear from Figure 3.1, NCI funds were increasing relative to the entire NIH allocations, but now from Figure 3.3 it can be seen that even relative to the other major institutes it was fairing quite well.

But the National Cancer Institute also had reasons to feel slighted in the years prior to the Cancer Act. Figure 3.4 displays the percentage of NCI grants, both in numbers and in dollars, of all NIH grants from 1946-1974. The Shannon

years (1955-1968) were years of decreased cancer funding relative to the growth of NIH. Numerous new institutes had been formed during these years and their funding demands had cut into the percentage of biomedical funds devoted to cancer research. Given the broadened institute structure and the shifts in funding priority during the 1950's and 1960's, it is quite understandable why the funding policy changes in the 1970's should create tension. With more institute "mouths" to feed, combined with an overall decrease in biomedical research allocations (Figure 3.1), non-cancer research was threatened by the war on cancer. Some areas of knowledge were being heavily funded while other areas were losing support. Even those institutes which were experiencing increases in their funding allocations during the 1970's were not increasing as rapidly as NCI. Absolute and relative deprivation was being felt by all except those involved with cancer.

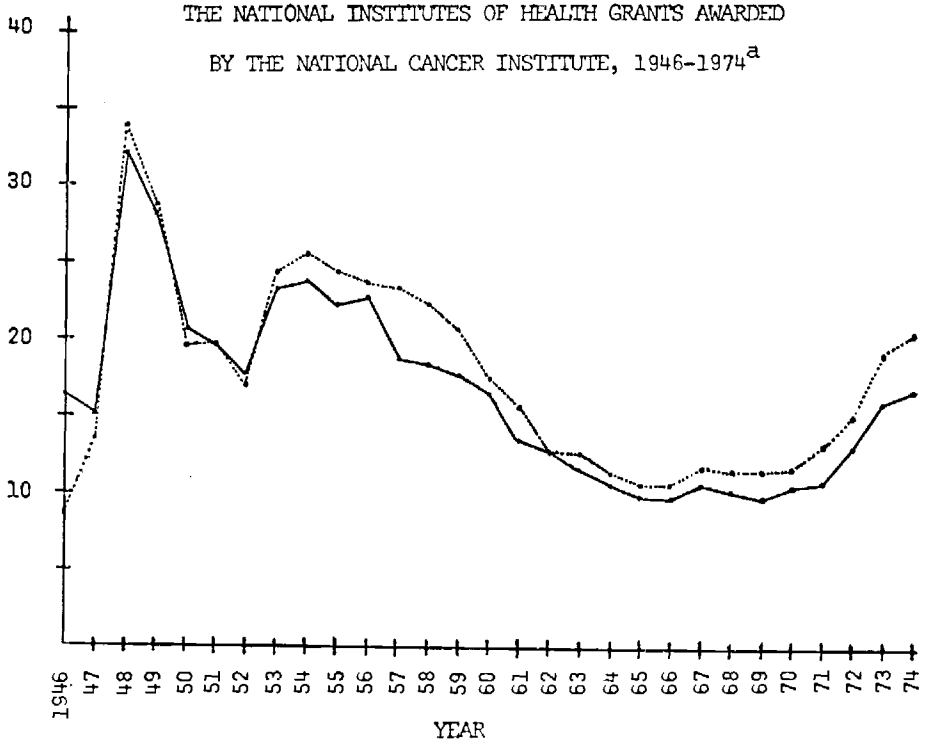
From Figure 3.4 one can see another source of tension surrounding the funding of cancer research. The National Cancer Institute had for some time distributed larger grants than had normally been the case with the other institutes (see Tables 3.2 and 3.3). This tendency toward the concentration of grant support continued to increase after 1973: 20 percent of NIH funds were given to 17 percent of the NIH grants. But the real cause for concern on the part of some biomedical researchers can only be seen if the changing contract structure, in addition to the granting structure, is taken into account (Rauscher, 1974; Kalberer, 1975).[27] For the decade of prosperity enjoyed by NCI has brought to a head the philosophical clash over strategies for funding biomedical research. To contract or not to contract for knowledge: that is the question.

Knowledge from Mission Money? Pro and Con

For some, the steady rise of "contracted" projects within NIH signals a major intellectual shift, and therefore, an encroachment on biomedical research.

FIGURE 3.4

PERCENTAGE OF NUMBER AND PERCENTAGE OF AMOUNT OF THE NATIONAL INSTITUTES OF HEALTH GRANTS AWARDED BY THE NATIONAL CANCER INSTITUTE, 1946-1974[a]



——— Percentage of number (number of NCI grants / total number of NIH grants.

•••••••••• Percentage of amount (amount of NCI grants / total amount of NIH grants.

[a]Compiled from National Institutes of Health (1975:117).

TABLE 3.2

NCI CONTRACTS AND GRANTS, 1962-74[a]

(Amounts in Millions of Dollars)

Year	Number of Grants	Amount	Amount/ Grant	Number of Contracts	Amount	Amount/ Contract	# Grants/ # Contracts	$ Grants/ $ Contracts
1962	1914	47.501	.025	107	24.3	.227	17.888	1.955
1963	1792	54.530	.030	266	26.3	.099	6.737	2.073
1964	1650	55.735	.034	316	30.3	.096	5.222	1.837
1965	1514	56.531	.037	267	37.6	.141	5.670	1.503
1966	1482	64.327	.043	250	33.1	.132	5.928	1.943
1967	1481	71.018	.048	294	40.0	.136	5.037	1.775
1968	1335	71.599	.054	364	41.2	.113	3.667	1.738
1969	1228	73.003	.059	367	41.4	.113	3.346	1.763
1970	1182	71.371	.060	566	43.5	.077	2.088	1.641
1971	1214	88.392	.073	686	71.0	.103	1.770	1.244
1972	1504	120.555	.080	843	116.8	.139	1.784	1.032
1973	1804	157.196	.087	1058	132.8	.126	1.705	1.184
1974	2241	219.846	.098	1600	199.2	.124	1.401	1.104

[a]Compiled from National Institutes of Health (1975:117f).

TABLE 3.3

NIH (EXCLUDING NCI) CONTRACTS AND GRANTS, 1962-1974[a]
(Amounts in Millions of Dollars)

Year	Number of Grants	Amount	Amount/ Grant	Number of Contracts	Amount	Amount/ Contract	# Grants/ # Contracts	$ Grants/ $ Contracts
1962	13061	324.598	.025	50	2.7	.054	261.220	120.221
1963	13441	376.378	.028	121	9.7	.080	111.083	38.802
1964	13592	442.189	.033	172	9.9	.058	79.023	44.666
1965	13669	482.232	.035	138	8.8	.064	99.051	54.799
1966	13671	536.646	.039	273	18.0	.066	50.077	29.814
1967	12456	522.295	.042	413	43.8	.106	30.160	11.924
1968	11785	554.419	.047	721	56.2	.078	16.345	9.865
1969	11207	554.578	.049	832	60.9	.073	13.470	9.106
1970	10157	530.782	.052	1540	61.8	.040	6.595	8.589
1971	9849	587.852	.060	1932	83.4	.043	5.098	7.049
1972	10020	684.486	.068	2166	133.2	.061	4.626	5.139
1973	9510	657.998	.069	2785	164.0	.059	3.415	4.012
1974	11170	857.331	.077	2449	160.2	.065	4.561	5.352

[a]Compiled from National Institutes of Health (1975:117f).

Longo (1973:2080) has pointed out that from 1971 to 1972, contracts in NIH increased by 47 percent but research grants by only 19 percent. The concentration of this funding mode in the new mission oriented programs (46.4 percent of NCI and 27.7 percent of "heart" funds administered in 1973 through contracts) was particularly visible to this critic. Why does he find this trend alarming? Longo replies:

> Of perhaps less general knowledge are the recipients of the largest contracts...Of the 10 largest NIH contracts, 7 were awarded to organizations other than universities. The data are perhaps even more startling when one looks at total contracts by various organizations. Of the five largest contractors, only one is a university. Of the 12 organizations with total contracts over $3 million, only five are universities and one the National Academy of Sciences. Forty-seven percent of NCI contracts were with profit making organizations in 1972. Route 70S near Bethesda, MD, is rapidly developing into a biological Route 128 composed of industrial contractors nourished by NIH. This trend, stemming from a quick-solution psychology, tends to remove research from the university and award it to industry. It remains unclear to what extent 'cost-plus research' by profit making corporations will deprive academic scientists of funds to pursue fundamental queries...It is clear, however, that well-motivated scientists must provide for themselves and that they can find reasons to shift allegiances toward contract funding, especially if the squeeze is tight enough and long enough. Contract research, which is largely for product delivery or procurement purposes, has the potential of undermining a scientist's commitment to patient, systematic and often frustrating discovery-oriented basic research (Longo, 1973:2080, italics added).

Contracts thus represent for Longo an approach to research which conflicts with certain well established views concerning the nature of the discovery process within biomedicine. It seems unfair to his argument to say simply that it represents academic versus industrial, production-oriented, values or basic versus applied or clinical research (see Gordon and Marquis, 1966; Cotgrove and Box, 1970; Comroe and Dripps, 1976). The real problem is the nature of the discovery process and the manner in which biomedicine progresses ("cognitive norms") and whether or not the contract mechanism is a threat to this development ("rhetorical norms").

The events which have precipitated such criticism can be clearly seen in Tables 3.2 and 3.3. By comparing these tables one notes that contracts have always played a more important part in the research effort of NCI as compared with the rest of NIH. While the trend is clearly toward more contract research in general, NCI is already expending virtually 50 percent of its external research monies on contract research. This rate is almost five times greater than the rest of NIH. This coupled with the fact that NCI in 1974 accounted for 40 percent of all NIH contracts and 55 percent of all research money expended through contracts reveals the basis for concern. If one simply shifted monies from one institute to another and allowed the internal research allocation mechanisms to operate, then the overall funding structure of NIH would increasingly resemble that of the favored institutes. In other words, by highlighting the funding of NCI research, as the war on cancer did, the internal patterns of research expenditures, likewise, gradually began to dominate NIH policy. The war on cancer thus precipitated a more rapid shift toward contract research than would have occurred through more gradual evolution. As one of our interviewees put it:

> You know, the point of the whole Cancer Institute is that it grew too fast. And without any decent quality control. It turns out a lot of people knew that and knew what they were doing. Which was simply taking advantage of a positive political climate in order to get as much as they could. And when you get it, you have to spend it. If you give it back, they are not going to give you as much again. So they were quickly finding ways to spend lots of money. And what [present NCI director Arthur] Upton's got to do now, and I think he recognizes that, is revamping things so that the amount of money that they have can now be utilized better, effectively building on the political clout or threat of his predecessors to restructure the Institute so that it uses that money effectively. Because we're under an enormous amount of pressure.

Part of the pressure "back then" was translated into NCI's mandated commitment to fund non-academic research. Some of the largest contractors were Litton Industries (22 contracts in 1972 totaling $16.5 million, Microbiological

Associates (12 contracts in 1972 totaling $7 million), and Flow Laboratories (9 contracts in 1972 totaling $4 million) (Longo, 1973:2080), all private laboratories which are heavily involved in NCI cancer research. These profit-making laboratories have been intimately associated with the war on cancer since its inception (see Anonymous, 1971a), e.g., Litton Bionetics, a subsidiary of Litton Industries mentioned above, received in 1972 a $6.8 million contract to renovate the biological warfare facilities at Fort Detrick for cancer research (Anonymous, 1972). Microbiological Associates, a subsidiary of Dynasciences Corporation which in turn is a subsidiary of Whittaker Corporation, has been very active in developing and producing tissue cultures as well as reagents for viral and immunological research for the cancer effort. Flow Laboratories, which dominates the European market of "biological products" through its laboratory in Scotland, has long been a "commercial leader in the virology field" so that it could easily shift when the war on cancer began to "studying the role of cancer viruses as potentially causative agents in human cancer"[28] (Monaghan, 1974:19-20).

The point that must be emphasized is that the contract mechanism did signal a shift in organizational focus of biomedical research and NCI can legitimately be given credit for hastening the shift by emphasizing non-academic contract research. Contract research was rapidly becoming a symbol of targeted research which was executed outside the academic setting by quasi-governmental research laboratories. The National Cancer Institute and its war on cancer was perhaps the single most important instrument for establishing this new organizational image for biomedical research.

The question once again arises: are the new organizational forms amenable to the structure of knowledge within the biological sciences? The concern of the scientist must center on the nature of the discovery processes within biomedical fields and how this interfaces with the goals that society would like to attain. If the policy maker is asked to heed this esoteric rationality--e.g., confluences

of knowledge--then perhaps the technical input-output models devised in and applied to other fields are indeed inappropriate for biomedicine. As one biochemist puts it,

> The current supports at NCI and the National Dental Research Institute for crash programs for solutions to cancers and dental caries are dangerous, in that they raise false hopes for solutions to problems for which an insufficient basis of knowledge is available. An example is the long-standing expensive Cancer Chemotherapy program which has had only slight success. It is based on the 'pill concept'; every disease can be cured if the right pill can be found (Pigman, 1973: 1733).

The contract, mission-oriented type of research represents for many biologists not just a violation of institutional norms, but the violation of cognitive norms, of epistemologies ("how do I know, learn, and discover within biomedicine") which, in turn, implicate some fundamental etiological understandings about the biological world. The etiological conflicts seem to be a particularly resilient bone of contention, for if one acknowledges that biological existence is an evolving, systematic phenomenon, then the quest for <u>any</u> causal explanation (as is often latent in mission-oriented research) can lead to a kind of "sectarian" science. Pigman conveys the difficulty in the chemotherapy tradition of cancer research; the "pill concept" or some other quick technological fix can distort and perhaps hamper the development of research programs in biomedicine (Zubrod et al., 1966). NCI's discouragement of research on the viral induction of cancer in 1938 (see Chapter 1) and its official reinstatement with the establishment within NCI of the Laboratory of Viral Oncology[29] are excellent illustrations of changes in etiological emphasis. Whatever the merits of this emphasis (as embodied in the Viral Cancer Program), some regard the vast sums of support for a viral etiological explanation of cancer as the epitome of sectarian science.

Reordering Cancer Priorities

With Frank Rauscher's retirement as NCI director in 1977, a cloud of doubt hangs over the future place and value of virus research in the cancer program. Some (Culliton, 1977) predict a shift in emphasis to environmental carcinogenesis research and a corresponding deemphasis on viral oncology. Others lament that with Moloney's ouster, the VCP is slowly being dismantled. On the fate of the Virus Cancer Program, American Cancer Society Executive Vice-President Rauscher hopes that

> it increases and expands. Again, recognizing that it's not the Virus Program that it was in 1966...Rather than detract from that Program, they ought to increase it. I know of no other way even conceivable right now, in which we can prevent many cancers in common denomicator ways despite the fact that we're breathing carcinogens right now and we're going to continue.... There's no doubt about the fact that the environment out there will continue to be contaminated for some time. We're not going to do away with the automobile, the asbestos in its brake linings, very quickly. So it's good to try and to talk about cleaning up the environment, cutting down on emission, and so forth. I'm very much for that, but we have to find a better way of living with things that we really don't want to do without, or can't do without, such as that form of transportation. We're also talking about a hundred different diseases that people call cancer. Cancers are formed, we think, by many different mechanisms, so we can not, it seems to me, look for a hundred ways of preventing these diseases. We've got to look for some common denominator ways of preventing cancer despite the fact that we're going to continue to be exposed to carcinogens for our bad habits. We shouldn't smoke, but fifty-five million of us do, and we have to worry about those brethren, too, as a matter of fact. The identification of pieces of the virus, of RNA viruses, or of genetic information, and the only thing that can code for that, as far as we know in all of biology, is an RNA tumor virus.

With the switch to specific program goals and a contract orientation to cancer research, there can also occur concomitant pressures for the adoption of (a) specific treatments, e.g., chemotherapy, or (b) certain causal explanations, e.g., viral etiology. It is the switch from broadbased support of

basic research to the concentrated betting on certain cures or causes that creates tensions for scientists with cognitive orientations to cancer other than those which currently enjoy popularity and political power. Given the necessity of explicit policy decisions in a national mobilization effort such as the war on cancer, there is always a danger that organization will lead to the perception of out-right "governmentalization" or the "politicization" of science[30] in the form of privileged research traditions or approaches. As Rettig (1978:4) recently stated in testimony before the Senate Subcommittee on Nutrition;

> ...the research effort that came after 1971 legislation looked very much like the effort that preceded it. Only is was on a grander scale. Though an internal planning effort of substantial proportions was undertaken, that effort had relatively little connection with the resource allocation processes and did little to set priorities for a period of resource scarcity. Programs that were important before the Act remained important afterwards. But concern for environmental carcinogenesis, for instance, emerged forcefully only in the mid-1970s after the identification of and attendant publicity about asbestos, vinyl chloride, and other chemicals as occupational and environmental hazards of cancer-causing potential. And that concern emerged primarily from sources outside the national cancer program rather than from within it. It is my impression, furthermore, that the concern for the relationship of nutrition and diet to cancer shares a similar history.

So in 1978 we hear echoes of the same allegations made about organizational priorities in 1974:

> Bad feelings about the VCP exist because there are a lot of virologists who share the same goals. The ones in the VCP were very rich. The others, who are just as good were very poor (James Watson in Culliton, 1974:144).

Watson, an outspoken proponent of funding for basic research,[31] draws qualified support for his position from two unlikely sources--the "fortuitous" cancer war team of Benno Schmidt and Frank Rauscher:

> We need to have a cadre of very good scientists just following their noses and not worrying about relevance. I felt very strongly about that, too; and I had no opposition, therefore, from many of my people, certainly not from the Congress. So there's a place for an ivory tower, but not at the expense of transferring technology

> that might benefit people.... The thing that I feared most was that as funds became tight and as there was a reaction against the cancer establishment because it was privileged status--this was absolutely predictable five or six years ago, but as that happened, and as it's happening now, the first thing that Congress would do, I think, would be to attack the image of an ivory tower privileged government laboratory, and they would delete funds from our own in-house organization or operation. I felt that would be devastating, really, to the national program, not only to NCI. So now that NCI is involved in technology transfer, in cancer control programs, I don't think there are many in the Congress who could say, 'Those guys out there just want to be left alone; they want to be funded simply because they are scientists; and they don't give a damn about Johnny with leukemia out in my district.' They can't say that because there's an honest-to-God commitment on the part of most of those people to help in various aspects of the National Program, be it review, site visiting, advice to the Director, advice to people on the outside, advice to the Congress, for that matter. They're heavily involved, more so than any other institute, and I think that's healthy (Rauscher, 1978).

Even more telling is the language of Benno Schmidt's fifth report to the president on the National Cancer Program:

> There is no question that there has been during this period an enormous extension of our science base and our knowledge as a result of the vast amount of highly excellent fundamental basic research that has been supported. But this extension of our knowledge only underlines how vast are the areas of ignorance which remain. Just as the past five years have brought a greatly enlarged science base, they have also brought important improvements in the clinic in dealing with cancer, but here again our progress only serves to emphasize how far we have to go...[we] cannot afford not to support basic research...For we are, in truth, profoundly ignorant about the real nature of cancer (quoted in Rettig, 1977:319).

The promise of 1971, fortified by massive mission money, has carried the Cancer Act to reauthorizations in 1974 and 1977. Since fiscal 1972, NCI appropriations have cumulated to over $5 billion, but the rhetoric has subsided. Today, Schmidt's report is restrained; Rauscher speaks of "over-promising" and "over-expectancy," and a reverse transcriptase researcher comments on the politics of cancer:

> ...the whole Yarborough Committee operation that set in motion the war on cancer occurred before the RT was known....RT fit very comfortably into their idea that there was new progress in cancer research that was exploitable...It was certainly, then, used politically a lot. There is no question. And, in a sense, appropriately, because it did represent the first opportunity to deal with a class of viruses that everybody knew were important, and no one knew how to deal with. And up to the time that we discovered RT, the amount of sensible work on RNA tumor viruses was miniscule before it was discovered. And just by providing a tool, never mind about providing a concept, it changed (overnight) the whole ability of handling these viruses. And since Huebner had imbued everybody with the belief that these were the key to cancer, there was no question that this was an enormous political, as well as scientific, breakthrough. The tough thing is to really say to what extent it mattered.

CONCLUSIONS

If 75 percent of all biomedical research carried out in U.S. medical schools and over 40 percent of all university research is funded by NIH (Gustafson, 1975:1060), then through shifts in policy such as that embodied in the war on cancer NIH can exert tremendous pressures on the selection of research topics.[32] Scientists will pursue the opportunities which increased funding makes possible.[33] But there is more. For our discussion of modifications of funding mechanisms within cancer research has suggested an interplay among normative structures by which scientists abide, but which they must also manipulate to protect their vested intellectual or organizational interests. By listening to criticisms by scientists one can quickly ascertain their concerns qua researchers. The norms to which they appeal are cognitive norms of argumentation within biomedicine and not the institutional norms which sociologists are enamored of belaboring as operative or obsolete. They may at once be both, yet this possibility is less compelling than the rhetoric scientists employ to communicate the policy-laden tensions to which their intellectual processes and products are now subject.

> Whether regarded with Panglossian optimism or Faustian foreboding, science is increasingly vulnerable to forces that

> intrude on its boundaries, permeate its social organization and expose its internal contradictions. Yet these forces may also bring about a more realistic awareness of the interpenetration of science and the social order (Nelkin, 1975a:27).

The social organization which money begets can be studied by conventional sociological means, but without an understanding of the normative structures, institutional, cognitive, and rhetorical, which guide and rationalize scientists' behavior, analysis of social organization is rendered hopelessly incomplete.[34]

Accordingly, by weighing both the discovery processes within areas such as cancer research and the criticisms which scientists have directed toward organizational tensions, sociologists of science can rethink their research tasks. The structure of reasoning can be pivotal in defining areas for study (see Chapter 2), while the search for violations of cognitive norms can translate the criticisms which scientists articulate into vital research questions about their "vocabularies of justification." For what we have here is not just Big Biology and contract research; what we have is ideology. And in Gouldner's (1976:36) words:

> It is one of ideology's essential social functions - of considerable cognitive relevance - to stand outside of science itself, and to reject the idea of science as <u>self</u>-sufficient or <u>self</u>-grounded. In other terms, ideology's critique of science, its refusal to let science be theonly judge of itself, its public exposure of science's selfishness,...and the <u>limits</u> of science, mean in effect that: ideology functions as an epistemology of everyday life.

For the biomedical researcher, science policy has created a new rhetorical vocabulary to vouchsafe the epistemology of their everyday science.

POSTSCRIPT TO PART I

Perhaps the most difficult hurdle for sociological analysis of biomedical problem domains is the nature of the interdependence of scientific knowledge and social organization. If the organization of science can distort and even thwart the development of science (as numerous scientists quoted above

seem to think), then how can one ever know what the biological argumentation process should be? This question can be reduced to the perennial quandry: can one ever progress from what "is" to what "ought" to be? The answer is, of course, no, if one assumes that both the "is" and the "ought" only represent different aspects of the same social system. All becomes relative from such a perspective on the social system or, for the case in point, the social system of science. But by considering the possibility that the development of science should not be equated with the development of the social system of science, one begins to fashion a realist theory of scientific growth which captures the force of knowledge. Knowledge seems to develop in spite of its formal and informal social structures. This can occur because of the "predominance of cognitive orientation" (Böhme, 1975:241) which scientists share and which shapes the parameters of their intellectual work.

By recognizing the predominant knowledge function of science, a norm is established by which a meaningful analysis of science can proceed. Like all knowledge, sociological knowledge of scientific development must be tested and retested to eliminate as much "noise" as possible from its theoretical and empirical understanding (see Mendelsohn, 1977). The sociologist must learn to separate the organizational "distortions" of science from the organizational forms which facilitate scientific growth and development. If, as a consequence of this argument, sociologists are forced to inform themselves of the content of scientific specialties then the payoff is that sociologists can speak forcefully to science policy questions. The "is" of the scientific reasoning process which develops in spite of, but which can be gravely thwarted by, the social organization of research, can then provide norms--institutional, cognitive, and rhetorical--for the "ought" of science policy.

Through a series of arguments the theoretical groundwork has been installed

for the following chapters. First (Chapter 1), the unfolding of a research tradition, cell transformation and the confluence patterns which precede discovery were discussed. Second (Chapter 2), by demonstrating how meaningful "slices" of biomedical research can be isolated for sociological analysis, it was seen that biomedicine features a "structure of relevance" or distinctive rationality for the formation and study of problem domains. Third (Chapter 3), we examined how scientists routinely separate in their own thinking the organization of science from the structure of argumentation which advances their research, and how funding policy and the rhetoric of the mission can facilitate, deter, but surely alter the advance of knowledge.

With these perspectives in place, we are prepared to survey the intellectual history of reverse transcriptase, a domain of research within viral cell transformation, and then analyze quantitatively the various local, sectoral, and international configurations of collaboration and research organization within the cancer community. This is the challenge of Part II: bring the biomedical rationality we have discerned to bear on the interpretation of growth and specialization of a problem domain. In so doing, perhaps a new policy orientation to the social study of science shall emerge.

NOTES

[1]The widely acknowledged architects of that policy--a coalition working from within and without the government in behalf of the Cause--were prime congressional movers John Fogarty and Lister Hill, NIH Director James Shannon, and the tireless champion in the private sector (notably the American Cancer Society), Mary Lasker. For an assessment of Lasker's role see Rettig (1977: especially Chapter 2).

[2]The National Cancer Act of 1971, Public Law 92-218, 92nd Congress, Senate 1828, December 23, 1971. This was followed by the National Cancer Act Amendments of 1974, Public Law 93-352, 93rd Congress, Senate 2893, July 23, 1974.

[3]As Jesse Steinfeld, then Surgeon General, testified before the Senate health subcommittee deliberating on S.34 (the blueprint for the Conquest of Cancer Act), "...scientists are like other people, they tend to go where the funds are, where the opportunities are, and it is conceivable that if we spend an enormous amount of money in the cancer program that people who might be more productive in other programs would move to cancer programs..." (U.S. Senate, 1971a:55).

[4]The National Heart and Lung Institute did not have long to wait, however, before they would also be singled out for special funding. In 1972 the President signed the National Heart, Blood Vessel, Lung, and Blood Act (U.S. Senate, 1972; see Culliton, 1973).

[5]Again, in the words of Rettig (1977:14-15):

> The conflict between the fundamental research strategy and the categorical disease strategy, then, actually masks five closely related issues. What kind of research is to be supported or favored--basic or clinical? What instrument of support is to be used--the grant or contract? Who is to make the authoritative decisions allocating support--the external scientific community, the professional staff of an institute, or the advisory council to an institute? Who is to be supported--university scientists or industrial researchers? What is to be the extent of formal research planning--limited, significant, or very extensive! This potpourri of issues was basically rolled into one in the debate over the National Cancer Act of 1971. The overarching issue concerned the most appropriate strategy of research management for conducting the war against cancer.

Specifically, the 1970 report of the Panel of Consultants called for a "comprehensive national plan" for cancer. Toward this end, then NCI director Carl Baker initiated an effort to develop the National Cancer Program. As Louis Carrese, then Baker's assistant and now Associate Director for Program Planning and Analysis, describes it:

> We brought together the whole scientific community, 250 people selected out of two thousand names submitted by every professional society in the country. Then we had 40 planning sessions to develop the National Cancer Program plan. The people who were there doing it

were fighting the process. During the very time they were doing it [over a four-month period], they were fighting it.... It's like the drowning person who says, 'Dear God (if there is one), save my soul (if I have one) from hell (if there is one).' So some of these guys were saying, 'I'm going to participate in this national planning session, but I will maintain my integrity as a scientist. You know, if the plan is fine, all right. It it isn't I've knocked it enough.' This process is, well, one of the most fantastic things I've ever witnessed. We turned it over to the community; we were very low profile by design.

In Rettig's (1977:300) view,

> The plan, though the object of much concern and criticism, has proved useful in explaining the cancer program to the Congress and the public and in providing general directions for NCI. It has not been used to any significant degree in governing the actual day-to-day management of various NCI programs. Neither Schmidt nor Rauscher have displayed more than mild support for the management importance of the plan itself.

[6]This appears to be a particularly difficult request to make of sociologists. For as Bohme (1975:215) has argued,

> What sociology of science has generally failed to do is to account for the primacy of cognitive orientation with regard to the social organization of science. In contrast to this, the organization of science was constructed solely on the basis of motivational orientation: thus Storer took the demand for creativity and competent reaction to be fundamental, and Hagstrom the system of sanctions. To be sure, the organization on the level of motivation is a reality, but it presupposes organization on the cognitive level.

[7]As Carrese (1978) told us:

> ...our major problem here was to convince people that, first, in addition to the traditional support of many bench investigators, we should try other things. The cancer problem is certainly large enough to accommodate more than one approach to try and solve it, and these would not supplant or replace things, but these new approaches would be complementary and supplementary, and introduce ways for us to think on how best to distribute and invest the total resources we've got across a whole spectrum of activities. Not just basic research, but other kinds of research, other kinds of developments, and now cancer control, with the passage of the Act.

[8]Indeed, as Rettig (1977:171) points out, the AAMC and FASEB (the Federation of American Societies for Experimental Biology) "did most of the work mobilizing the academic medical-scientific community to oppose S.34."

[9]Schmidt has been the chairman of this panel since its creation in 1971.

[10]Speaking at a "retrospective" on the Cancer Act of 1971 in 1976, Harold Amos, an NCI stalwart in the academic sector, put it this way:

> All investigators in the biological sciences are agreed that serendipity is their most valuable ally. What emerges unexpectedly in experiments is often the most critical information obtained in the experiment and those findings are especially pertinent to new directions in approach and understanding, throwing new light on old questions. It is imperative that the state of mind of the investigator be such as to perceive the unexpected for what it may ultimately be worth. Program relevance dictates a selection in registering of observations that may categorize as worse than useless a contradictory finding (Amos, 1977:262).

[11]Mulkay (1976) stresses this very point in recognizing that the rhetoric of scientists' pronouncements or "vocabularies" vary with the audience they are addressing. Such a strategy serves multiple purposes, e.g., maintaining distance between the scientist-experts and the lay public, and promulgating the search-for truth ideology as a rationale for decrying impediments to the flow of research dollars.

[12]Testifying before the Rogers subcommittee, Baltimore himself argued that

> Cancer should not be separated from the rest of biomedical research, and a crash program atmosphere should not be created, because...the American people should not be misled into thinking that a cure for cancer is imminent (quoted in Rettig, 1977:235).

In view both of the rhetorical excess surrounding the cancer legislation and the political side of the debate with which the American Cancer Society was aligned, it is ironic, as Baltimore (1978) commented to us, that

> The American Cancer Society has an extremely honorable record supporting basic science, from way back. And lots of people will tell you, lots of people who do work on bacteriophages and general problems of molecular biology, will tell you that the key support that they got was from the American Cancer Society at a time when NIH wouldn't touch them. The Cancer Society has had very far reaching effects, very good panels, and has very good luck with its approach toward its goal of research; in fact, more so then than now, now that they are caught up in the climate of 'Let's get cancer cured.'

[13]It appears however, that this Report of the Ad hoc Review Committee of the Virus Cancer Program was never officially accepted by the Board. As late as February, 1978, the report was not catalogued in the National Library of Medicine. So its status remains somewhat of a mystery to us, although it is cited as "submitted" in draft and final report forms as November 1973 and March 1974, respectively, in Rettig (1977:369). The reports we secured through the courtesy of Dr. Rauscher's office at ACS bear these same dates.

[14]Todaro and Huebner were quick to deny any wrongdoing in this policy or their role in implementing it when we queried them--separately--about it.

[15]The first criticism we suggested to Rauscher was an intellectual one, essentially that mentioned in the draft of the Zinder report itself:

> It was the assumptions that were wrong. There did not, nor does

> there exist, sufficient knowledge to mount such a narrowly targeted program. Basic ignorance of the mechanism involved in the cancer process, even in animals where a viral etiology is definitively established, is so profound that it is difficult to be certain where to begin, much less organize a focused attack.

[16]Litton Bionetics "manages" the Frederick Cancer Center at Fort Detrick for NCI. There is more than a little ambiguity as to which researchers are on which payroll, a problem Rauscher and other of our interviewees readily acknowledged.

[17]Again, in justifying the past, but assessing the present situation (now as an ex-NCI spokesperson), Rauscher (1978) confides:

> One other reason, incidentally, we use the contract mechanism so strongly in our own Viral Oncology Program, is we were able to award contracts in something like 2 or 3 months, with peer review. Right now, it's taking the NCI something like 12 months to award a grant, and almost 16 to 18 months to award a contract. Nobody wants a contract anymore. The people at NCI don't want to manage contracts. It's just too much regulatory red tape. But we could use contracts at that time to abet a Congressional decision to give us money to get on with the virus cancer search. You couldn't do that with a grant.

[18]An example of the animus expressed by clinically-oriented M.D.s toward the basic scientists who opposed S.1828 is this excerpt of a letter from the medical director of St. Jude's Children's Research Hospital received by Rep. Rogers:

> They were 'far removed from the sick,' had 'little appreciation' of what was involved in turning scientific findings into 'effective prevention and treatment of disease,' and 'have not and never will take part in direct application of scientific research ot the health of the American people.' The choice before the subcommittee... was whether it wished to 'represent American science or the American people' (quoted in Rettig, 1977:361).

One clearly hears the rhetoric, though it rings hollow.

[19]The recent recombinant DNA controversy and NIH deliberations on security guidelines for university laboratories have brought representatives of the scientific community into encounters with a formidable "external" power--city councils determined to minimize the biohazards in <u>their</u> communities.

[20]For a good survey of shifts in funding policy as well as levels of funding within the National Cancer Institute after the National Cancer Act, see Kalberer (1975). For a comparable analysis of the whole NIH for the decade <u>preceding</u> the Act see Kennedy et al. (1972).

[21]It would be interesting, for instance, to see how these dollar "votes" of concern for cancer correlate with such phenomena as the increased numbers of elderly within the population (i.e., more individuals in higher cancer risk categories), and with macro-economic (recessions) and -political (wars) events (see below).

[22]This fear has also been translated into massive Mary Lasker-Ann Landers-inspired-letter-writing-to-your-congressman campaigns, as that which was urged when S.34 was nearing a vote in April of 1971.

[23]The role of the American Medical Association during the gradual displacement of private support of governmental programs for biomedical research is of particular interest; the AMA remained aloof. With the passage of the National Cancer Institute Act four decades ago, the AMA warned that "The danger of putting the government in a dominant position in relation to medical research is apparent" (Strickland, 1972:14). On the "neutrality" of the AMA toward medical research, Strickland (1972:154-155) states:

> For a long time it was as though the organization which represented most of the thousands of practicing physicians had vacated the field of medical research policy.... Ultimately, the Association came to realize that there was a major and ironic incongruity in the fact that the organization claiming as a cardinal tenet the advancement of good health for all citizens had had nothing to do with the greatest effort of the century to make possible the attainment of that goal.

[24]Commenting on the Schmidt-Rauscher team as a "fortuitous" combination, one NIH official has said:

> [Rauscher] could cause a convulsion in NIH if he tried. A director who wished to exercise the full range of authority could beg a hell of a large degree of autonomy from NIH. But Rauscher had played it very carefully (quoted in Rettig, 1977:298).

[25]Several of our NCI interviewees stressed that the laboratory is the basic unit of the Cancer Institute (and all of NIH, for that matter), whereas the program is more flexible and perhaps more ephemeral in carrying out specific parts of the mission at various times. Rauscher (1978) agrees with this distinction, and that accurately reflects NCI planning.

[26]Along with mental health, cancer and heart (which is now called Heart, Lung and Blood) are also the only institutes with statutory obligations for disease control.

[27]Recall that the changing relationship between grants and contracts dates from 1965 and the advent of the Special Virus Leukemia Program.

[28]It is noteworthy that an NCI-Litton Bionetics team, Gallo (NCI), Yang and Ting (Litton Bionetics) were the first to find the enzyme reverse transcriptase (see Chapter 4) in human leukemia patients (see Anonymous, 1970a). The wedding of the private and the governmental laboratories has (as shall be seen in Chapter 5) created distinctive collaborative patterns within cancer research.

[29]This did not occur until 1971, thus it took NCI 23 years to change its mind and effect a new policy.

[30]One of the difficulties with analyzing the politicization of science is that all the readily available examples occur in political situations which are easily condemned, e.g., the case of T.D. Lysenko (see Medvedev, 1969). It would seem, however, that even the most gradual policy shifts can subtly politicize a research problem by establishing a reward system which is out of touch with the present

scientific realities (see Haberer, 1969; Ezrahi, 1971; Nelkin, 1975b).

[31]More recently, Watson has severely criticized those scientists who advocate a moratorium in recombinant DNA research (see Nelkin, 1978).

[32]Gustafson (1975:1063) estimates that "proposals to NCI now account for roughly half of all applications to NIH."

[33]This is, of course, an "externalist" view of history (introduced in Chapter 1). It would be useful to know if researchers have actually changed their research programs or merely altered their rhetoric to fit under the umbrella of the cancer program. See van den Daele et al. (1977:222-227) for a discussion of such "re-labelling" in science.

[34]Funding is often spurned by sociologists since it seems to explain everything about the growth of science on the macro-level, or it appears to explain nothing at all on the micro-level. It appears, on the one hand, to drive the system, but, on the other hand, its influences are so subtle that they are shrouded by other variables (see Orlans, 1971; Price, 1969).

A DEMOGRAPHIC PROFILE OF REVERSE TRANSCRIPTASE RESEARCHERS

INTRODUCTION

By now, the discovery of reverse transcriptase--from antecedents to outgrowths--constitutes yet another chapter in the history of science. Like other episodes of competition, multiple discovery, and subsequent bursts of activity and new knowledge, this history has been an unfolding research drama enlivened by several characters. The "characters" to which we refer are not only the most "public" of figures, the Nobel laureates Temin and Baltimore. Indeed, the cast includes the Spiegelmans, Greens, and a coterie of NCI luminaries who have infused the history of reverse transcriptase with a personality and intensity all its own.

Although the emphasis throughout our historical treatment has been on the science involved in reverse transcriptase--from the brilliant to the mundane--we have not forgotten the scientists responsible for the episodes; we have simply postponed analysis of "the cast" until now. The analysis we shall conduct is a blend of disciplinary approaches which have developed independently and for distinct intellectual purposes, yet share a common unit of analysis--the individual.

Historians of science, enamored of "great man" theories and the textbook heroes who seem to take quantum leaps in the quest for knowledge while the rest of us imperceptively inch along, have devised a method for retracing those leaps; it is called "prospography" or "collective biography" of elite groups of scientists (for a review, see Pyenson, 1977). Likewise, sociologists of science, preoccupied with productive, visible, academically-employed natural scientists, have pursued career patterns analysis or the demography of scientists, particularly the stratification and mobility of this technical

labor force (e.g., Harmon, 1965; Hargens, 1969; Folger et al., 1970; Cole and Cole, 1973; Zuckerman, 1977).

Despite the fact that both of these approaches celebrate the ultra-successes in science, they have been practiced in relative isolation of one another. Prosopography has featured small samples and anecdotal accounts of backgrounds, e.g., cultural and parental influences, while demography has been synonymous with large sample, quantitative analysis. In the former, the elite are deceased; in the latter, they are alive but unencumbered, i.e., they are not surveyed by mail, phone, or in person. Instead, their careers are seen "unobtrusively" as educational and job histories, augmented by measures of research performance.

While the "disciplined myopia" with which these respective tools have been applied is cause for contemplation (see Thackray, 1977), they each, in their multivariate breadth, have much to offer when invoked at the present research site. For our goal is to construct a set of biographical profiles which correspond to our several categories of "structurally interesting" persons in the history of RT. These profiles will "contextualize" the linkages between people and events by presenting backgrounds, tracing influences, and pausing to examine some idiosyncratic details that occupy the "backstage" of RT. This is as much an "ecological" approach--relating an aggregate people to an intellectually, spatially, and organizationally diverse environment--as it is demographic--following the movements of the aggregate and their ideas within this environment (see Duncan, 1959).

This analysis is also motivated by a sobering reality of science: like other workers, scientists are constrained by their work environment. Yet too often, as Whitley (1977:23) correctly observes, scientists are treated like "free agents" subject to no local whim, indeed, carving a career swath through an array of institutions which make few demands but liberally

dispense the rewards of promotion, remuneration, local status, etc. A more accurate portrayal--as the literature on creativity in work settings attests (e.g., Pelz and Andrews, 1976)--is that scientific work and its myriad expressions of productivity are fundamentally shaped--and comprised--by social, political, and logistical imperatives (see Hargens, 1974, for comparative data). The intellectual process, in other words, occurs in anything but a vacuum: it is collaborative, competitive, and part of a larger ongoing activity. The climate is established and maintained by the employing organization, and in accordance with its priorities and commitments. Ideally, scientists are accommodated by these organizational imperatives, but, then again, they are hired to help satisfy those imperatives and fulfill the commitments. Realistically, because research laboratories and academic departments are more stable than those who inhabit them, i.e., pass through as "intellectual commuters" (Price, 1963), these organizations exert pressures designed to maximize their success. If, in the course of work, scientists can subsume their goals under those of the organization, mutual goals may be achieved. Often, however, this is not the case. A coincidence of various means may never develop, and tensions may arise extending from the sacrifice of personal goals to the obstruction or subversion of organizational goals.

Such a range of "fit" between individual scientists and their organizational affiliations is apparent in the reverse transcriptase domain. For example, the glaring contrast in program foci, magnitude, and research style between NCI labs and academically-based labs--even the more visible and well-funded of the latter--invites some assessment of the scientists involved in various team efforts, the constraints under which they labored, and the ways they adapted or became reconciled to the kind of scientific work they were compelled to do.

Another question concerns the intellectual breadth of individuals and

teams. For most, reverse transcriptase represented but one problem deserving two, perhaps three, years of investigation, among many problems to be explored along, say, the virology or epidemiology trail. Whence did the RT researchers come--in an intellectual sense? What was their graduate training? And where did they go after contributing to RT? Evidence on these queries begins to put RT in the wider perspective of cancer and biomedical research in general. It also suggests the demographic and ecological patterns which biomedical researchers create, both in response to their innermost intellectual cravings and to the unique attractions which certain labs, programs, and institutions hold. It is through analysis of scientists in organizations, therefore, that the RT case study assumes a more inferential meaning--about intellectual migrations, research productivity, career vagaries, and the institutionalization of scientific work.

THE DATA

The subjects for biographical profiling are 58 RT researchers whose contributions to the domain were revealed (as discussed in Chapters 4-6) in several ways: prolific authorship or publication of highly (co)-cited pre- or post-discovery articles, membership in a visible NCI or academic lab team, and frequent acknowledgment as a reader, supplier of materials, etc., in RT articles. These structured criteria, of course, were used for selecting scientists as interview targets. Hence, our 15 interview subjects are among the 58. Twenty-six others were sent letters soliciting a current curriculum vitae and 18 complied (a 69 percent response rate). In addition, dissertation abstracts were located for 31 North American Ph.D. holders in the Comprehensive Dissertation Index. The main source of biographical data, however, was American Men and Women of Science (12th and 13th editions). Indeed, 43 of 51 (or 84 percent) of the U.S.-based members of our purposive

"structural" sample were listed in this directory, as testimony to their accomplishments and status relative to the scientific community-at-large (see Crane, 1965). To augment these data with information about overall research performance (not just publication in RT), we consulted the Source Index of the Science Citation Index and coded articles published into three periods: pre-discovery, 1965-69; post-discovery domain development, 1970-74; and domain transition (i.e., "current" applications of the enzyme), 1975-76. Note that these periods refer to stages of RT activity, although the articles coded encompass all serial publications spanning the 11 years 1965-76. In this way RT publication can be assessed vis-a-vis total research productivity.

As a further comparison, a second aggregate of scientists was defined as the complement of the structural sample: those who published in RT, published prior to the discovery, and were highly cited, or were often acknowledged in RT articles, but had not gained the recognition of inclusion in AMWS and for whom no other biographic information could be found. For these 41 scientists, the identical publication data were coded from the Source Index, so that comparative analysis of research performance within and without RT between this heterogenous "control group" and the purposive sample would be possible.

THE PROFILES

A Provisional Construction

No significant differences exist between the structural sample as a whole and its constituent subsamples in chronological or professional age. For all 58 scientists, their mean and median birth years are 1933 and 1936 respectively, while the mean year in which they received their highest degree is 1963 (median = 1966). If we consider Ph.D.s only (n = 37 or 64 percent), or those with M.D.s (n = 16), or both the Ph.D. and M.D. (n = 5),

no deviations obtain. Thus, the principal researchers in RT were, on average, 34-37 years old (± 9 years) at the time of the discovery of the enzyme and had barely concluded a quarter of their projected 35-year postdoctoral career. For many, therefore, the advent of RT came during service in first professional positions, a "period effect" (Ryder, 1965), which we examine below in terms of subsequent research interests and productivity.

Returning to origins for a moment, however, we note that of the 55 scientists for whom Ph.D./M.D. institution information was secured, 15 were trained in 12 schools located in 10 different foreign countries. Ten of these foreign-trained scientists took postdoctoral positions in visible U. S. labs, e.g., Spiegelman's and Baltimore's, and most of these found permanent jobs in academic departments and private institutes, e.g., Sloan-Kettering and the Salk Institute. Among the 40 American-trained scientists, 30 different institutions granted the M.D. or Ph.D. with only the University of Illinois (4) and Washington University (St. Louis) (3) awarding three or more such degrees. The doctoral fields most represented--irrespective of national origin--are biochemistry and microbiology.

Although the period of transition from initial research interest to involvement in reverse transcriptase was brief for most of the RT principals, two kinds of migrations can be traced to reveal shifts in careers. One migration is intellectual (see Chubin, 1976:459f for a review), namely, current field of (self-) identification compared to field of highest degree, as reported in AMWS. Table C1 summarizes this professed migration or change of identification. The most striking pattern discernible in the sparse data of this table is that five years after the discovery of reverse transcriptase, 15 researchers (37.5 percent) regard virology as their primary field identification; this includes 5 of the 12 M.D.s. That 11 researchers would have gravitated to this field suggests a real intellectual attraction or "pull"

TABLE C1

MATRIX OF FIELD IDENTIFICATION AT TIME OF PH.D./M.D. RECEIPT
AND IN 1975-76 FOR PRINCIPAL REVERSE TRANSCRIPTASE RESEARCHERS

PHD/MD FIELD	FIELD IN 1975-76					n
	Microbiology	Biochemistry	Virology	Molecular Biology	Other[b]	
Microbiology	4	-	1	-	1	6
Biochemistry	-	3	1	2	-	6
Virology	-	-	4	-	-	4
Biology (unspecified)	-	-	3	-	-	3
Chemistry	-	1	1	-	1	3
MD	2	1	5	2	2	12
Other[a]	-	-	-	1	5	6
n	6	5	15	5	9	40

[a]Zoology, genetics, biophysics, physiology, and anatomy.

[b]Zoology, genetics, biophysics, anatomy, chemistry, immunology, bacteriology, pathology.

SOURCE: American Men and Women of Science, Thirteenth Edition (1976)

effect. This is supported, indeed amplified, by the specializations and research interests listed at the end of the AMWS biographies. Although the number and terminology of these responses are open-ended, a coding of key-words reveals that three-fourths of the biographies contain "viruses"; one-fourth mention "RNA viruses" in particular. Other variants include "viral oncogenesis," "oncornaviruses," "tumor viruses," and "replication of viruses."

Still another dimension of professional identification is society/association membership. With multiple responses possible, one-half of the researchers listed affiliation with the American Society for Microbiology, a third with the American Association for Cancer Research, less than a fifth with the American Society for Biological Chemistry or the American Chemical Society.

A second kind of migration, that between employment sectors (e.g., Crowley and Chubin, 1976), may also be indicative of shifts in career plans and goals. To examine this possibility, we constructed Table C2. Significantly, we observe virtually no inter-sectoral migration from the time of discovery to the peak period of domain growth. What is obscured on the diagonal is the amount of within-sector institutional migration. Other data (on median time employed by an institution) suggest that such movement is fairly common (an average of once every three years). Apparently, however, the work contexts distinguishing the three sectors--or the barriers which preclude entry and exit from them--are sufficient to retain research personnel. Several of our interviewees stated, in fact, that "academic researchers wouldn't work" in NCI labs,[1] and, likewise, "NCI researchers can't afford to go back to academia, even to a well-endowed medical school." Clearly, the rewards differ in each setting, making a traversing of the boundaries separating academia from nonacademia a rare occurrence (only 4 cases in Table C2). What slippage does occur seems to stem from an M.D.'s desire to enter clinical

TABLE C2

MATRIX OF EMPLOYMENT OF REVERSE TRANSCRIPTASE RESEARCHERS IN THREE SECTORS, AT TIME OF DISCOVERY AND DURING PEAK DOMAIN GROWTH

	1972-74 SECTOR			
1969-71 SECTOR	Academic/Medical School	Government	Private[a]	n
Academic/Medical	26	2	1	29
Government	-	6	-	6
Private	1	2	7	10
n	27	10	8	45

[a]Includes hospitals.

Chi/square for academic vs. nonacademic = 29.88, 1 df, $p < .001$

practice or a scientist's wish to teach part-time instead of preparing contract proposals and researching full-time.

What the profiles presented thus far hint at are distinctive career trajectories for subsets of RT researchers. In general, these researchers are professionally young[2] and somewhat intellectually mobile. Many entered the RT domain through postdoctoral apprenticeship at large academic labs[3] or a shifting of research interest while at an NCI lab. The major referents for their work circa 1975, as measured by specialization and professional society membership, are tumor virology and related microbiological and biochemical applications to cancer. Furthermore, their lack of mobility between employment sectors and the not-inconsiderable status attained in their positions by 1975[4] suggests that even the NCI wunderkinder have forsaken the research role[5] (see Zuckerman and Merton, 1972). Indeed, most seem to be thriving in research, having made the adjustments necessary to capitalize upon local resources while satisfying administrative demands and, at a microscopic level, lending ecological stability to the RT domain. To explore these impressions systematically, however, we must relate the emergent career paths to other research activities coterminous with the growth of the RT domain.

Productivity in Reverse Transcriptase and Beyond

The measurement of research activity within the RT domain represents only a portion of the samples' overall productivity. The questions we now address center on changes over time in the proportion that is devoted to RT, and the correlation of productivity measures with various institutional and career-phase attributes. For this analysis, we compare four samples: the structural (n = 58), the control (n = 41), those scientists located only

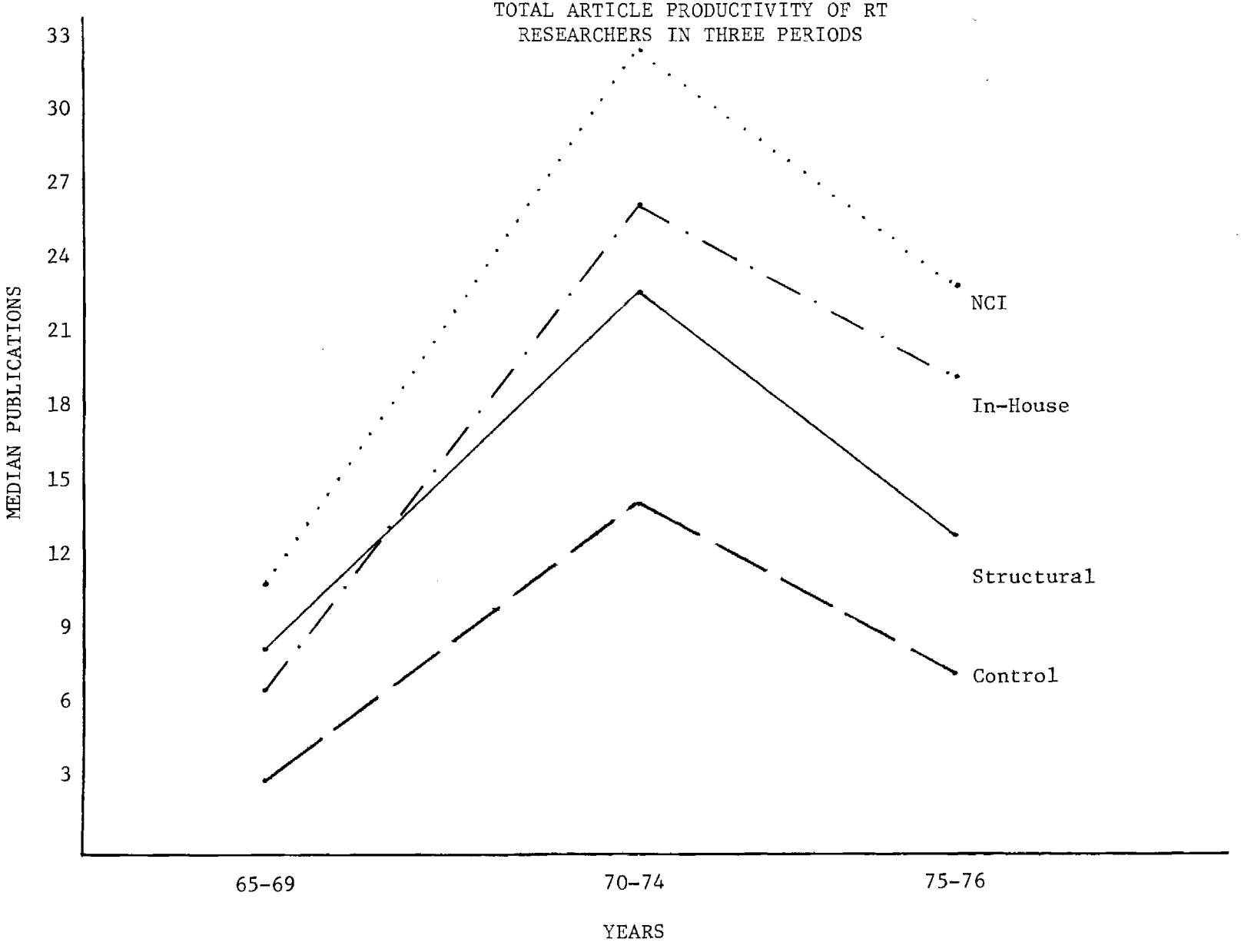
at NCI labs from 1970 to 1974 (n = 12), and the members of the visible in-house network (n = 16).[6]

As seen in Figure C1 the shape of the total article productivity distributions is almost identical for the four samples, though the level of effort in each period varies markedly with the most productive NCI sample outstripping the median output of the least productive control sample by margins of 4, 2, and 4 to 1, respectively in the three time periods.

It is in Table C3, however, that the disparities in productivity both within the RT domain and relative to total article output are in full display. The first row of this table suggests that volume of productivity in the period preceding the discovery in 1970 is a poor predictor of RT productivity for all except the in-house sample. Conversely, the zero-order correlations for all except the control sample indicate that during the two post-discovery periods, RT publication is significantly correlated with total publication output. Nevertheless, if median RT publications are divided by median articles for 1970-74 (the interval basically covered by the RT effort and our article set), we see that the proportions for three of the samples hover around one-third. Only the in-house network devotes a majority of its effort to publication within the RT domain. This is not surprising, of course, since the productivity criterion for admission into this network was at least 10 articles, a threshold, you recall from Chapter 5, satisfied by only 23 scientists, a scant 2 percent of all the authors identified as publishing on reverse transcriptase. Admittedly, therefore, it is this criterion, in part, which assures the high correlations for the in-house sample. The magnitude of those correlations reinforces the visibility accrued to these 16 by virtue of their coauthoring together in the domain. Their median output of RT articles is more than double that of the eminent structural sample, and even overshadows the voluminous contributions by

FIGURE C1

TOTAL ARTICLE PRODUCTIVITY OF RT RESEARCHERS IN THREE PERIODS



SOURCE: SCI Source Index, Institute for Scientific Information (1965-77)

TABLE C3

MEASURES OF ARTICLE PRODUCTIVITY WITHIN THE RT DOMAIN RELATIVE TO TOTAL PRODUCTIVITY, BY SAMPLES

	Structural (n=58)	Control (n=41)	NCI (n=12)	In-House (n=16)
Pearson r between RT articles and all articles in--				
1965-69	.153	.252	-.220	.478[a]
1970-74	.502[b]	.078	.496[a]	.600[a]
1975-76	.415[b]	.185	.703[b]	.524[a]
Median RT articles	6.9	4.4	12.5	16.5
Median RT articles ÷ all articles 1970-74	.288	.301	.385	.634

[a] $p < .05$

[b] $p < .005$

SOURCES: SCI Source Index and RT article file.

NCI researchers. What these data thus convey is the greater preoccupation with reverse transcriptase research by the in-house and NCI samples.[7] Both in an absolute and a relative sense, these researchers devoted a larger fraction of their effort to the problems raised by the discovery of the enzyme. Indeed, the members of the structural sample concentrated the smallest proportion of their output (.288) in 1970-74 on topics within the domain, a finding which prompts conjecture about the wider scope of their respective research programs and, in terms of their career histories, the status they brought with them to the domain rather than that which they derived from it. For the members of the structural sample, as well as for the control aggregate, research on reverse transcriptase may well have represented more of a "passing interest" than a "going concern." We probe for the validity of these conjectures in the multivariate analysis which immediately follows.

Explaining Productivity: A Discontinuity of Effort

As a final attempt to determine the relationship of domain productivity to total research effort, we shall perform a series of linear (ordinary least-square) regression analyses. Inspection of the zero-order and partial correlations among demographic and ecological variables resulted in entering degree year into the equations (due to high collinearity, $r > .8$, with birth year), as well as two dummy variables--degree type (Ph.D. or M.D.), and academic/medical school only employment (1965-76). In the equations predicting RT productivity, none of these variables had more than a miniscule effect,[8] though "academic only" employment does appear in the regressions reported later.

In Table C4, the number of RT articles published by the structural and control samples are regressed separately on three career variables. Significantly, membership in the in-house network is a better predictor of within-

TABLE C4

REGRESSION OF RT PRODUCTIVITY ON THREE CAREER VARIABLES FOR THE STRUCTURAL AND CONTROL SAMPLES

	STRUCTURAL		CONTROL	
	r	β	r	β
Total 1965-69 articles	.153	.188	.252	.204[a]
Membership in in-house network	.675	.702[b]	.781	.723[b]
Location at NCI lab 1970-74	.355	.031	.462	.079
R^2	.436		.624	

[a] F-value $p < .05$

[b] F-value $p < .001$

domain productivity than 1965-69 publications. In fact, the in-house variable--though in part a proxy for RT publication--accounts for 90 percent of the variance explained in each of the equations. The low magnitude of the zero-order and standardized regression coefficients for the structural sample is further evidence of discontinuity in research effort, namely, that the level of productivity by these 58 scientists prior to the discovery is a poor harbinger of productivity within the RT domain. But does the reverse obtain? That is, does RT productivity accurately predict the level of research output in 1975-76? Or does a linear combination of other variables provide improved explanation?

To pursue these questions, the regression analyses summarized in Table C5 were performed. Proceeding from the most specific publication information--RT articles (see Table C5, Regression A) to the most recent 1970-74 articles (see Regression C)--we observe a three-fold increase in explained variance in the structural sample and a five-fold increase for the control. These data confirm the conventional wisdom that the best predictor of publication at t is publication at t-1. But notice the significant β for membership in the in-house network for the structural sample under Regression B, and the effect of location in an NCI lab for the control sample under B and C.[9] Work environment in the case of NCI researchers does contribute to their productivity level in 1975-76. Academic/medical school employment--perhaps because it is less homogeneous in terms of size, resources, and work obligations--exhibits no explanatory power.

Once again, then, number of RT articles are far less predictive of subsequent overall productivity than earlier, i.e., prediscovery, publication. This finding lends validity to our earlier conjecture about the discontinuity in level of research effort. Contributions of the two samples to the unfolding microcosm of RT seem to be one aspect of ongoing programs that

TABLE C5

REGRESSION OF 1975-76 ARTICLE PRODUCTIVITY
ON PUBLICATION AND CAREER VARIABLES FOR
THE STRUCTURAL AND CONTROL SAMPLES

REGRESSION	STRUCTURAL		CONTROL	
	r	β	r	β
A.				
RT Articles	.415	.433[a]	.185	.146
In-House Network	.225	-.091	.142	-.236[a]
NCI Location	.222	.194	.423	.486
Academic Employment	.020	.191	c	c
R^2	.198		.134	
B.				
1965-69 Articles	.694	.872[b]	.569	.620[b]
In-House Network	.225	.261[a]	.142	-.220
NCI Location	.222	-.078	.423	.580[b]
Academic Employment	-.020	.022	c	c
R^2	.542		.532	
C.				
1970-74 Articles	.804	.892[b]	.808	.764[b]
In-House Network	.255	-.051	.142	.013
NCI Location	.222	.047	.423	.304[a]
Academic Employment	.020	.111	c	c
R^2	.651		.726	

[a] F-value $p < .05$

[b] F-value $p < .001$

[c] No information for this sample.

center on, if our earlier characterization of AMWS specializations is correct, tumor virology and the cellular mechanisms underlying cancer.

CAREER PROFILES: RAMIFICATIONS AND LIMITATIONS

Reverse transcriptase, as a research problem and domain of activity, derives coherence from the large laboratories which commit their resources and energies to it. This commitment is by no means exclusive; other research not dependent on the enzyme appears to be maintained simultaneously.[10] Hence, the connectedness of the domain to other research sites, even as seen through the productivity of the central RT scientists, labs, and networks, is of negligible assistance in identifying those other particular sites. One thing cna be stated with certainty: affiliation with an NCI lab promotes overall productivity. Regardless of whether this large laboratory effect inflates visibility in the biomedical literature,[11] as demonstrated in Chapters 5 and 6, it undoubtedly furnishes the wherewithal and institutional support of readily-available collaborators to seize upon and extend the discoveries made in smaller, modestly-endowed and -manned labs. This seems to be the legacy of Big Biology and perhaps the clearest benefit of mission-oriented research. As several of our interview subjects asserted, "The discovery of reverse transcriptase could not have been made in a large NCI lab because you just don't have time to think imaginatively."

If discovery is in the realm of basic unfettered science, then, in contrast, intramural NCI researchers are best equipped and poised to strike deeper at the targets hit by extramural researchers. We hasten to add, however, that most NCI scientists with whom we spoke confessed that their initial reaction to hearing of the discovery of reverse transcriptase was not surprise or shock, but, "So what? Big deal!" Most claimed to be

vaguely aware of the provirus hypothesis at the time, but utterly unconcerned with the repercussions that its confirmation would have.

One conclusion to be drawn from our findings, then, is that a "eureka model" of biomedical specialization still very much engenders the attraction of researchers to new problems.[12] Neither degree type nor professional youth predict where biologists will migrate in search of new problems or new angles on old problems. The traces of a problem domain that we detect in a concentration of literature must be extracted from the larger intellectual nexus in which it is enmeshed.[13] Ironically, what we extract is so intimately connected to so much that, upon separate analysis, the domain now isolated harbors few clues as to where to look next. For just as researchers become acclimated to a particular work environment, they are fickle in applying their energies to an assortment of problems; they preserve the setting for their research, while re-ordering their intellectual priorities. Thus, when we examine a problem domain, we see a patchwork of fragmented individual, institutional, and perhaps even sectoral (e.g., government) research programs, while the "missing" fragments of these programs attach to other literatures.

Likewise, a domain captures a cross-section of the biomedical community--in terms of career trajectory, disciplinary background, scientific role incumbency--that has converged sufficiently long on a problem to attract new resources, attention in the literature, funding from agencies, and manpower from those capable of shifting gears on sustaining multiple interests. This represents a fluidity of careers for some, a status change for others, a sudden wave of productivity, visibility, and prestige for many. Such a period effect is bound to be lost at the formal, official, or on-the-record level. For this reason, we sought to collect--through interviewing--informal, subjective, and private perceptions.

To wit,

> I've heard of people who were disappointed because they didn't get the Nobel prize. When they . . . started working on RT problems after the discovery, they then thought that they should somehow be prized. I just thought that was silly.
>
> Any small investigator would be crazy to start working on RT [after the discovery] . . . knowing that he was going to have to compete with these huge laboratories [e.g., Spiegelman's, Gallo's, Todaro's]. . . .
>
> . . . the guys who got the Nobel prize for this thing are the ones who have issued by executive fear . . . that RNA tumor viruses have nothing to do with human cancer. Right? This has been taken very seriously by the guys who dole out the money. And so young people who just don't have the security required to stick with their convictions are just not applying for money [to do] research on a human disease. . . . It makes life difficult in the sense that I don't get any intellectual and experimental support in the community. . . . [T]hat means everything has to be done in one place . . . and is slowed down enormously. So instead of getting help and cross-fertilization in terms of ideas and data coming out of the laboratories, that's just dried up.
>
> I guess I consciously decided after the discovery of RT that I wanted to take that as an opportunity to get deeply involved in studying RNA tumor viruses and the whole problem of cancer. And being a lab scientist, I thought it most appropriate to let the experiments take me. . . .

CONCLUSION

A demographic profile of RT researchers is a collective biography of a set of scientists constrained by many realities: a discovery, diverse disciplinary frameworks and training experiences, and jobs demanding the meshing of intellectual impulse with deadlines, personalities, and often mundane obligations.[14] This set of biologists--those we interviewed and those we merely glimpsed at a distance--are remarkably accomplished, insightful, and engaged in what we suspect are modally productive careers. Their coming together for a short time in a problem domain is just one in a succession of such confluences they will experience and help to foment. Yet we must be resigned to understanding mere segments of their intersecting professional

lives spent in particular domains, or abandon the confluence approach altogether and embrace a neater cohort design wherein the individual *sui generis*, and not the specialty or problem domain, commands the focus of analysis.

Because we have opted for a confluence approach, the individual is relegated to a subordinate role as an agent of change and symbol of success in the historiography of a domain. By comparing two and sometimes three and four small aggregates of researchers whose career paths crossed within the RT domain, we have sampled a population which, in a host of ways, impinged upon, and became identified with, the specialization of reverse transcriptase. We know now, too, that it is indispensable to that historiography to track ideas through persons and organizations; idealized scientific objects alone--theories, methods, and apparatus--will not do. Such objects are manipulated and debated, socially transformed into structures that social analysts can then dissect and relate in time and space to a wider spectrum of esoteric artifacts and their specially-trained producers.

This is a realistic approach to science (see Epilogue) in which the force of knowledge--within a domain and beyond--begets a demography and ecology of science which affects both the state of knowledge in biomedicine and the more idiosyncratic processes of scientific careers. Thus, our methodological injunction persists: large-sample unobtrusive sociological analysis of career patterns and small-sample intellectual and social history cannot stand alone in explaining migrations to and from problem domains. Motivations, intentions, and responses to organizational imperatives cannot be inferred from the operation of period effects on aggregates of scientific workers.

The subjectivity of career decisions must be probed with the subjects

themselves[15] (see Goodfield, 1977), ideally but realistically in the context of (a) their natural work environment, and (b) the research program which dictates their manipulations of cognitive objects (see Whitley, 1977). This recourse to the living is why the problem domain is especially accessible; nonetheless, a domain is easy to reify and difficult to interpret apart from the problem which generated it. That is, reverse transcriptase is not the sole province of virology or molecular biology, because the problem is really "cancer" or "cellular transformation"--problems which require multiple disciplinary perspectives and occupy correspondingly heterogeneous cadres of researchers. To divorce the problem from its attendant work force is to separate cognitive from social structure. And to separate these structures is to explain neither: a worker-less science equals science-less careers--an absurdity no social analyst can abide.

NOTES

[1]The most celebrated exception to this dictum would be Jeffrey Schlom's experience. A Rutgers Ph.D., Schlom did predoctoral research at NCI, was invited by then-NCI director Rauscher (himself a Rutgers Ph.D. in virology) and John Maloney to join Spiegelman when he moved from Illinois to Columbia (in search of a medical school and clinical climate). Spiegelman invited Schlom and a productive four-year collaboration ensued, whereupon Rauscher "called" Schlom back to NCI to chair the Breast Cancer Virus Segment of the Tumor Virus Detection Section in the Laboratory of Viral Carcinogenesis. Like Aaronson, Gallo, Scolnick, Todaro, and Parks (who has since left NCI to practice pediatrics), Schlom "grew up" in NCI. All are young men in their mid- to late-30s who now hold key administrative positions while remaining "at the bench."

[2]The only "elder statesmen" of the domain are Spiegelman and Huebner, both in their 60s, and Green, who is only 52.

[3]This is consistent with Crane's (1972) findings on the attraction of young scientists to visible senior researchers in mathematics and sociology specialties, but underscores the import of studying the postdoctoral mentor-student relationship (see Zuckerman, 1977, and Mullins, 1973, for evidence on the predoctoral version of this relationship).

[4]Among the mail respondents, 14 report (median) advisory service on two editorial boards of major journals such as Journal of Virology, Cancer Research, Cell, and Journal of Molecular Biology. In all, the honorific-functional role of gatekeeper (Crane, 1967) was being executed for 21 different journals.

[5]As one of the wunderkinder with whom we spoke put it:

> In an Institute like NIH and NCI, to get really top-flight scientists to become administrators at these high levels, you really have to be fairly dedicated and interested in these broad, broad things, because in most universities you can make a lot more money as you move up towards the deans and, you know, presidents; and in institutes like Sloan-Kettering or McArdle or some of these other places, as you move up, that doesn't mean you move out of your lab relationships. In a government situation, beyond the level that I'm at now, which is lab chief, if you move higher than that, you're involved in sort of distributing money to the world. You really can't, I don't think, compete at that point. You have to be above the battle, so it isn't that easy to fill these higher positions.

[6]Note that whereas the first two samples are mutually exclusive, the latter two are subsets which cut across the combined membership (n = 99) of the former. Specifically, one half of the in-house network membership were NCI employees from 1970-74.

[7]Interestingly, if only first-authored articles are considered (a measure Zuckerman, 1968, has shown to indicate junior status, i.e., its frequency wanes with increased professional age), three-fourths of the NCI sample's median output in the 1965-69 period qualifies, a proportion almost twice that of the structural sample. In the post-discovery period, these proportions decline

to a similar level of one-third, but the median number of the first-authored articles by NCI researchers is 11.8 as compared with 10.5 for the in-house sample and 8.5 for the structural sample. In short, youth and the visibility achieved via first authorship in collaborative publication need not go hand-in-hand.

[8]In addition, type of degree is inconsequential for predicting productivity. Apparently, a research M.D. and a Ph.D. are comparable, though the orientation and focus of their respective researches (even within a problem domain) may differ. Our suggestion that a medical-clinical model may be guiding the work of M.D.s more than basic Ph.D. scientists was pooh-poohed unanimously by our interviewees. Distinguishing orientations is not that clean or simple.

[9]A similar effect was revealed in the question using 1970-74 articles as the primary independent variable for predicting 1975-76 productivity of the in-house sample (R^2 = .763).

[10]See Hagstrom (1970) for a discussion of simultaneous work and publication in different but related research areas. Such simultaneity seems to contrast with the migratory pattern manifested by other scientists (see Edge and Mulkay, 1976: Chapter 10).

[11]Belatedly, we report that the confounding of individuals by laboratory citation profiles militated against our use of citations as a variable in the previous regression analyses.

[12]A quote from one of our subjects ably summarizes the attraction phenomenon:

> You know, at some point in your life, you become fairly wedded to a given area. You've done well, you know it. But the young guy, the guy coming out of school, you know, if there is money, he'd be a food not to look at it. If you just haven't become too focused yet. I think that does attract.

[13]As one of our interviewees put it:

> All you have to do is read the Proceedings of the National Academy of Sciences and find several good areas which are being minimally explored, which you can do experiments on, and which will be contributory.

And yet another claimed:

> You ask anybody in what section of PNAS their paper was published, and they probably will not be able to tell you--whether it was microbiology or biochemistry or genetics. I mean, they don't know.

[14]For example, one of our subjects, a veteran academic researcher, commented on NCI employment:

> If you are at NIH, first of all, or NCI, which is the same thing, you have some security in that you have a laboratory and a stable source of money. However, you have a problem that you can't expand your laboratory and bring more people in, so you have to go and get some other building someplace run by somebody else under a

> contract, so you're going to have to do all the administration. In addition, at NCI you get paid less, so you have to be somewhat dedicated, and furthermore you're given tasks. The tasks are that if you have these contracts, you have to go out, and as part of your obligation, write reports about these contracts, defend those contracts, evaluate them, present them to other study sections, so there is a lot of scientific administrative work that goes along with it. So it's sort of a mixed blessing being at NCI.

The minority viewpoint is that of prominent lab chief:

> I came from an academic setting, and if I ever left NCI, I would go back to an academic setting. I consider myself a reasonably good scientist in standing with the academic community, but I am told that actually their perception--the academicians' perception--of the way government does business in fact is not the way that the government does business.

This relates to another lament we frequently heard concerning the inflexibility of Civil Service: the security it affords the lab workers can haunt the lab chief because civil servants can't be fired; only minor realignment of such personnel is allowed. As one NCI chief assured us,

> If one wanted to get rid of someone who was obviously incompetent, one would have to give up science for a year and make it a full-time desire.

[15]A flurry of biographical and autobiographical accounts has recently appeared which trace the evolution of personal research programs against the backdrop of intellectual and social histories of disciplines. These memoirs, taken together, offer new opportunities for weighing the forces which shaped scientific careers and ostensibly alter the face of those disciplines as well (see Bernstein, 1977; Kuhn, 1977; Merton, 1977; Toulmin, 1977).

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